

REMINISCENCES
OF THE
MULLARD SPACE SCIENCE LABORATORY
TO 1991

DR E. B. DORLING

PROLOGUE

“There is properly no history - only biography” - Emerson

On the 4th of December 1954 the SS Queen Mary left Pier 90 in the New York docks for Southampton carrying a full complement of passengers, most of whom, I imagine, were looking forward to being home for Christmas. Amongst them my wife, Clare, and I were glad to be on the last leg of our way back from a less-than-satisfactory stay of fifteen months in Vancouver, British Columbia. What awaited us in the New Year was still unsure.

I was already thirty. I had come out of the Army in Germany in 1947 to go to Bristol to read physics. After six years there, and without a clear idea of what to do next, I had felt that to travel was better than to arrive, and had taken a research fellowship at the university in Vancouver. I quickly realised my mistake. For my wife, a qualified teacher, Vancouver was not in welcoming mood—it had too many teachers. Try as she might she could find no work. As an experimentalist I had been expecting as a matter of course to join a group with some on-going research effort, but the “group” consisted of one member of staff—no research in progress, one large, empty, underground laboratory, one small annexe. I was free to do what I wished, but with few resources. I made the best of a bad job, found some apparatus for the development of infrared detectors that had been abandoned by an earlier visiting Englishman, and did what I could. Vancouver, in the mid-1950s, had no indigenous light-engineering industry, nothing in the electronics field. The economy was centred on timber, agriculture, and various ores mined up the coast or in the interior. Scientific equipment—if there was money with which to buy it—came from the UK or from the States. In either case it took months to arrive. I stayed for as long as seemed proper under the circumstances and then came home.

University posts in this country were hard to come by, and badly paid. At Keele, the first new post-war university, I was offered an assistant lectureship at £650p.a. Had I taken it, my wife and I would have been required to live on the campus in staff quarters for which the rent was, I seem to remember, £500p.a. At Nottingham I asked for a lectureship, but that was not on offer either. Whilst my search went on I taught physics at a small evangelical public school, and in many ways enjoyed the experience. The headmaster was keen to have me stay on, but the head of physics was less encouraging. I was not there to teach physics, he explained, but to use physics or any other subject as a medium for supplying the boys with a Christian education. It would have meant a radical change of heart on my part.

An advertisement caught my eye—the Civil Service Commission was inviting applications from physicists no older than 31 for entry into established posts. With my 31st birthday only months away I saw one of the largest employers of physicists soon to close its doors on me. I applied, and in July was appointed as a senior scientific officer. Once appointed, it was a matter of finding a home, one with a suitable vacancy. To me the obvious place was the Radar Research Establishment at Malvern with research interests closest to mine, particularly in the infrared field, but I was politely turned away; I had no electronics experience. Next it was Farnborough, the Royal Aircraft Establishment, and in particular the Guided Weapons Department. I met the head of the Guidance Division, but again the problem was my lack of electronics experience. I was about to take my leave when M.O. Robins, the head of Control Division, looked in for a quick word and was introduced to me. Did he have any suitable vacancy, he was asked. He couldn't think of one and left. I believe I was on my way to the door, resigned to having drawn a blank yet again, when Robins reappeared. ‘The Gassiot rocket project that we are about to start on—we shall need someone to liaise with the universities. Would that interest you?’ I had read a Royal Society announcement in the newspapers a day or so before describing the plans for a high-altitude rocket programme, so it was fresh in my mind. I seized the offer.

On September 12th 1955 therefore I became a member of Robins' Control Division, one of a small team in it newly headed by J.F. Hazell, charged with the task of designing and building the so-called Gassiot rocket. I knew nothing of the field and had a great deal to learn, but it looked most interesting. As it turned out, I was to spend the whole of my working life in it, mostly in a way I could not possibly foresee.

CONTENTS

1. THE BEGINNINGS OF SPACE SCIENCE IN THE UNITED KINGDOM.....	1
THE BACKGROUND	1
PLANNING THE RESEARCH PROGRAMME.....	2
SKYLARK	3
US ROCKET DEVELOPMENTS	5
2. THE START OF SPACE SCIENCE AT UCL	9
RESEARCH USING SKYLARK.....	9
COOPERATION WITH NASA	12
3. ARIEL 1.....	15
4. THE MOVE TO HOLMBURY ST MARY.....	25
FINDING A HOME.....	25
HOLMBURY HOUSE.....	25
I JOIN UCL.....	26
CONVERSIONS & ADAPTIONS	26
A FINANCIAL CRISIS	28
WATER, WATER, EVERYWHERE.....	30
THE FIRST NOEL.....	31
BUILDING WORKS BEGIN.....	31
THE FIRST FIRE ALARM.....	32
WATER AGAIN.....	32
THE FINAL MOVES FROM GOWER STREET	33
THE OPENING.....	34
WORKSHOP ORGANISATION	35
COMPUTING ARRANGEMENTS	35
WE GET A CLEAN ROOM.....	38
THE DIFFICULTY OF FORECASTING THE FUTURE	39
TWO SPECIAL VISITORS.....	39
5. INSTRUMENTS ON NASA SATELLITES	41
EXPLORERS 20 and 31	41
ORBITING SOLAR OBSERVATORIES	431
ORBITING GEOPHYSICAL OBSERVATORY	43
6. SCIR & SPLMS	45
STORED CHARGE IMAGE READER.....	45
SHORT PATHLENGTH MASS SPECTROMETRY.....	47
7. COPERNICUS	49
8. ARIEL 5.....	55
9. THE ESRO/ESA SPACECRAFT	59
ESRO 1 & ESRO 2.....	59
ESRO 4	59
GEOS 1 and GEOS 2.....	62
EXOSAT.....	63
FIREWHEEL.....	65
METEOSAT.....	65
10. MORE ROCKET EXPERIMENTS	67
THE SPORADIC-E LAYER	67
STABILISED SKYLARKS	68
HIGH LATITUDE ROCKET CAMPAIGNS	71
SMALL ROCKETS	71
CASTING YOUR BREAD UPON THE WATERS	72
11. ULTRAVIOLET ASTRONOMY	73
12. ARIEL 6 - THE LAST OF THE BREED	77
13. MORE ESA SATELLITES.....	81
GIOTTO	81
HIPPARCOS	82
14. THE SOLAR MAXIMUM MISSION	83
15. TWO SPACELAB EXPERIMENTS.....	85
SPACELAB 1 GAS SCINTILLATION COUNTER.....	87
16. ACTIVE MAGNETOSPHERIC PARTICLE TRACER EXPERIMENT (AMPTE)	91
17. THREE MORE SATELLITES	93
ROSAT	93
CHEMICAL RELEASE AND RADIATION EFFECTS SATELLITE (CRRES).....	94
ASTRO-C.....	95
18. REMOTE SENSING AND EARTH RESOURCES	97
19. THE LARGE SPACE TELESCOPE	99
EPILOGUE.....	105

1

THE BEGINNINGS OF SPACE SCIENCE IN THE UNITED KINGDOM

THE BACKGROUND

Space research using rockets began in the United States of America shortly after the end of World War II. US scientists made the first measurements of physical conditions in the upper regions of the Earth's atmosphere from captured V2 rockets. Thereafter US engineers started the development of purpose-built rockets with which scientists from US Government establishments and from university research groups could begin methodical investigations into a wide range of new and fascinating topics to do with the Earth, its surrounding atmosphere, the Moon and planets, the Sun, and all that lay beyond.

The United Kingdom's entry into space research began unobtrusively through the work of the Royal Society's Gassiot Committee. The committee had been set up to administer the Gassiot Fund for meteorological research; during World War II it was chosen by the Royal Society to organise research on atmospheric physics for the Air Ministry. Thanks largely to Sydney Chapman, this work became well established and after the war it expanded. The committee noticed the particular significance to atmospheric physics of the US rocket research. In the United Kingdom there were many physicists, epitomised by the late Sir Harrie Massey, FRS, then Quain Professor of Physics, Head of the Department of Physics at University College London, and a member of the Gassiot Committee, whose research interests, though necessarily confined to the laboratory, impinged directly on the problems then being actively pursued by the Americans. Unfortunately there was little contact between the two sides. Travel to the United States was restricted by the severe foreign exchange restrictions in the UK, and any discussions involving missile developments were inhibited by security considerations, that being as true in this country as in the USA.

It was Sydney Chapman who proposed that the Gassiot Committee should invite the US Upper Atmosphere Rocket Research Panel to a conference in England. The US Navy's Attaché in London, Dr Fred Singer, a University scientist on loan to the US Navy, used his unique position as confidant to senior Government scientists on both sides to advance this proposal. He arranged for 15 members of the Rocket Panel to come to a conference, chaired by Massey, at Oxford in August 1953. There they would discuss the US research effort with scientists drawn, through the good offices of the Royal Society, from British and Australian Government and University laboratories, and from other European countries.

For the British, thoughts of the planned meeting were spiced by the knowledge that Massey had been asked a few months earlier by an official of the Ministry of Supply whether he would be interested in using rockets available from the Ministry for scientific research. He had been able to say "yes" readily because, as head of a large and expanding physics department, strongly supported by the College and by the University of London, he knew that the necessary resources and enthusiasm would be forthcoming. Robert Boyd, now Sir Robert Boyd CBE, FRS, then a lecturer carrying out experimental research work on rates of electronic and ionic collision processes in gases using newly-developed probe methods, was particularly enthusiastic about the opportunity thus being opened up, and thereafter he led the UCL research effort.

An additional incentive at the time of the Oxford meeting was the International Geophysical Year, the 1958 IGY. It came from an idea first promoted in 1950 by Lloyd Berkner, head of the Brookhaven National Laboratory, at an informal meeting at James van Allen's home at Silver Springs, Maryland, with Sydney Chapman, Fred Singer and two geophysicists. It was taken up by the International Council of Scientific Unions. The Royal Society came to play a leading role in the IGY, and it was therefore inevitable that the UK moves towards a space research programme should have been linked in many minds with the IGY. In fact the two were independent but parallel developments.

The Oxford conference was most successful. Its proceedings were later published, Boyd being one of the two joint editors, Seaton the other. A major result of the meeting was a series of discussions between representatives of the Ministry of Supply and the Gassiot Committee, now chaired by Massey, about the possible use of rocket facilities at the new test range at Woomera, South Australia. The Committee referred the matter to one of its three sub-committees, again chaired by Massey, which concerned itself with the photochemistry of the atmosphere, and asked it to formulate proposals. These were endorsed by the Council of the Royal Society, and agreement between the Ministry of Supply and the Royal Society was reached early in 1955. In June 1955 Treasury support was confirmed. A sum of £100,000 was to be provided, spread over 4 years, to be shared equally between the RAE on the one hand for the rocket vehicles and the Gassiot Committee on the other for the scientific programme. The rockets would be fired from the Australian Weapons Research Establishment's range at Woomera, South Australia. WRE itself was located at Salisbury, near Adelaide.

The arrangements were released to the Press in August 1955. The funding announced by the Royal Society was widely interpreted as being for a single rocket flight as part of the IGY. The general public, and, for that matter, very many scientists who really should have known better, recalled the exploits of the Swiss Piccard brothers in ascending to almost 17 kilometres in a balloon in 1932, and descending to 3 kilometres in a bathyscaphe in 1953, and assumed that something similar was to happen here - a one-off manifestation at a very modest outlay. Mention of more than one rocket flight often brought the reaction - 'Why should there be a second?'.

PLANNING THE RESEARCH PROGRAMME

With the design and development of the rocket itself set to commence at the Ministry of Supply's Royal Aircraft Establishment, Farnborough (RAE), in mid-1955, thoughts turned to the organisation of the University research programme. The Gassiot committee, now formally responsible for making the arrangements, set up a further sub-committee, the Rocket Subcommittee or Subcommittee D, to advise on this programme. Amongst its founder members were six university scientists who were to play leading parts in the subsequent evolution of the programme, Massey, Boyd, D.R. Bates (Queen's University, Belfast), W.G. Beynon (University of Wales Swansea, and later Aberystwyth), J. Sayers (University of Birmingham) and P.A. Sheppard (Imperial College London).

Boyd, through a consultancy at RAE in the summer of 1954, had made sure of a head start by finding out just what might be achievable at Woomera. Much new tracking and telemetry instrumentation to follow a rocket making a ballistic trajectory to 100 km and higher would, he learned, have to be developed, installed and tried out by the Australians at Woomera and this would take time. RAE's performance estimates of the still non-existent rocket were based on little firm data, the solid-propellant motor having yet to be designed and manufactured, as had the nosecone, payload structure and fins. The rocket would eventually carry much standard tracking and telemetry instrumentation, but development from the existing systems into one

suitable for use in a new rocket, operating in a quite novel environment, was also going to take time. There was a launching tower to be designed at RAE, constructed, shipped to Woomera and erected. For every ten questions asked by the scientists, therefore, perhaps only one could be answered by the rocket designers at the RAE. It was a difficult time for decision-making, but a very exciting time to be planning a research programme.

Much later, in December 1958, the Royal Society set up a new committee, the British National Committee for Space Research, initially to be the national body adhering to the soon-to-be-formed International Committee for Space Research, COSPAR. Sir Harrie became the first BNCSR chairman and under his guidance the committee rapidly assumed responsibility for the coordination of the UK space programme, including the activities of the Rocket Subcommittee. The BNCSR then remained the authority for the scientific support of the UK space programme until sometime after the creation of the Science Research Council in 1965. A 'History of British Space Science' by Sir Harrie Massey and M.O. Robins, (CUP 1986), gives a detailed and authoritative account of these times.

The Rocket Subcommittee's work in mid-1955 led to recommendations to the Treasury for funds to support six experiments, by Bates and his colleague Emeleus, by Beynon, Sayers, and Sheppard, and by Boyd and his colleague G.V. Groves. The latter recommendation was for £1500 to UCL to support the determination of upper atmosphere pressure, temperature and winds on the one hand, and the determination of the ionic composition of the ionised layer on the other. The research interests of the UCL group were for many years thereafter closely linked with the fortunes of the RAE rocket from the very beginning of its development in the mid-1950s to the last flight in the UK programme in 1978.

Detailed planning of the experimental programme became the task of a technical committee made up of the University experimenters and representatives of the RAE group responsible for the rocketry. This committee met for the first time on 27 September 1955 in the Department of Physics at UCL with Boyd deputising as chairman for Harrie Massey. The newly-appointed head of the RAE group J.F. Hazell and I attended. Unknown to me, a liaison officer already existed in Dr W.G. Parker of the RAE's Rocket Propulsion Department at Westcott where the rocket motor was to be developed. He had had the benefit of a visit to the USA in June to discuss the design of upper atmosphere rockets and had been appointed to the Rocket Subcommittee, but over the months he found himself in the wrong establishment. Perhaps it was Boyd's awareness that Parker was to have been his contact with RAE that led him to identify me at that first meeting not as an ally but a competitor, for he was very frosty. He later explained that he had felt sure I was there representing a rival research effort!

SKYLARK

J.F. Hazell, though only days into his new job, was an experienced rocketeer with a good grounding in aerodynamics. Much of the ultimate success of the new venture was due to him. He was very cautious and very stubborn. For this he drew much criticism upon himself from outside, but his first thought was always to ensure a safe and reliable system. He had had a recent, unhappy experience of an explosion and serious injury to one of his colleagues working on a rocket at Woomera, and he had seen that same rocket encountering intractable aerodynamic problems. He was determined therefore that if, in a project openly visible to the general public through the University participation, caution and stubbornness were necessary to avoid trouble and keep the project 'clean' in the eyes of his superiors, that was how he would proceed.

It may seem odd that caution should have been so much to the fore in so new a field as space research, but it was only too necessary. There was little enthusiasm to be found for the project in high places; some seemed to resent having to host a project unconnected with defence. There

were difficulties even closer to home. 'Keep out of my sight and keep your noses clean,' a Head of Department once said to Hazell in a fit of exasperation, 'and I will leave you alone. Otherwise I shall close the whole project down.'

It had been one of my tasks, largely self-imposed, to write articles about the Gassiot rocket project. Security considerations made it necessary that everything be approved and there were misgivings, but eventually it was agreed that all would be well provided I gave no technical details of the rocket motor itself. I ran into trouble with officialdom when I mentioned the maximum speed reached by the rocket. Was I not giving away performance details of the motor? 'Can I keep the bit in about the peak height reached?' I asked. 'Of course'. 'Then that must imply the maximum velocity,' I said, skipping one or two important qualifications. 'How do you make that out?' came the reply. 'Its not me,' I said, unkindly, 'It's Sir Isaac Newton. He worked it out three hundred years ago.' 'I shall have to consult my colleagues,' came the unhappy reply. Soon all was pronounced to be in order.

The rocket was identified at the RAE as CTV5 Series 3, as it belonged for very practical contractual reasons to a fifth-generation project, a control test vehicle, and was the third in the series. There was no doubt in Hazell's mind that in due course CTV5 Series 3 was going to pose very novel control problems, but it was an uninspiring name. Couldn't we match the catchy name Aerobee which had been given to the new high-altitude rocket in the USA, I asked myself. Whilst Hazell was away with Boyd in Australia making plans for the first experiments, I got to work on a long list of names for the rocket, mostly quite impossible but ending with the one I wanted, Skylark. 'No. No. No,' said Mac Robins, my division head, as he scored his way patiently down the list, until he paused. 'Skylark'. That might do. But you'd better get it officially approved.' And on 10 July 1956 I was telephoned from headquarters; it was all in order. 'But remember,' came the warning, 'it is only a nickname. There is no change to the project name.' The name first appeared in an article I wrote for the September 1956 issue of the RAE house magazine.

Hazell was not impressed when he arrived home from Australia, but otherwise 'Skylark' was generally popular, and the name 'CTV5 Series 3' quickly disappeared. There was one dire consequence. As I had been warned, the project name had remained unchanged. When, therefore, orders began to appear at headquarters for quite major items of expenditure marked down to a project completely unknown there, they were consigned to some limbo. Most seriously, the supply of motor tubes was stopped and bitter reproaches were aimed at me.

The technical meetings at UCL continued for some years. All but the first were held in Massey's room at UCL. Ionospheric and atmospheric physics dominated the planned experimental programme, with Beynon, Boyd and Sayers proposing instruments with which to measure ion and electron concentrations. It was Beynon who at an early meeting brought home to me the relative dearth of ionospheric information then available. Ground-based vertical-incidence sounders (ionosondes) were the only available tool. He pointed out that they suffered from a serious limitation - nothing could be measured between the E-layer maximum and the point on the lower edge of the F1 layer where the ionisation density exceeded that at the E-layer maximum. To Massey the photochemistry of the ionosphere was the holy grail, but he realised that filling in the E/F gap was a target not only worthwhile but achievable. In any case Skylark at that stage offered little scope for anything more ambitious, either instrumentally or operationally.

The June 1955 announcement of funding for the rocket and its instruments, £100,000 over 4 years, provided a very satisfactory blanket under which space research in Britain might be conceived, but it in no way provided for the true cost which was to follow when the child was born. There were to be several infant years of uncertainty before, as we shall see later, the

BNCSR and the Ministry of Aviation reached agreement on a customer/contractor method of funding the Skylark programme.

US ROCKET DEVELOPMENTS

With the creation of NASA in October 1958 there was a temptation in Britain to look across the Atlantic and say that over there they manage things better. But one could well be overlooking that period in the US space programme, well before 1957, the year of Sputnik, when there was no clear directive about the exploitation of space for scientific purposes. The US space scientists and engineers started their programme after the war on personal initiative. The scale was, of course, much greater than that in Britain from 1955, not simply because the Americans were so much more adventurous but also because the USA had emerged from World War II into a boom, whereas we in Britain suffered ten years of austerity. But it would be wrong to imagine that the start of space research in the USA was planned from the top, for it wasn't.

The services in the USA, the Army Air Force and the Navy, entered the space business separately and with different motives. The Army Air Force effort had a predominantly military basis, which was revealed most publically when the Army stepped in and launched the first US satellite. It was the US Navy which, much earlier and quite surprisingly, started a rocket programme motivated by a desire to learn about the upper atmosphere. The story is told by Milton W. Rosen, the engineer in charge of the Viking project, in his book 'The Viking Rocket Story' (Harper and Bros, New York, 1955). In 1945 Rosen was on the staff of the Naval Research Laboratory (NRL) in Washington. He was an engineer in a team of physicists and engineers that had been working during the war on the guidance and control of missiles. They had read all they could on missile developments in Europe and now, with the war at an end, they were planning new forward-looking projects. It was Rosen who conceived the idea that one of these new projects might be upper-atmosphere research using rockets. He put it to his boss, Krause, himself newly returned from Europe where he had learned of the German rocket developments. Krause was enthusiastic, and it was he who thereafter found the money. On 1 January 1946 the NRL Rocket-Sonde Research Division opened for business. Its stated purpose was the exploration of the upper atmosphere with instruments carried in rockets. Research groups were formed covering cosmic rays, spectroscopy, the atmosphere and the ionosphere. J.W. Townsend, later a deputy director of the Goddard Space Flight Center, was deputy leader of that Division. 'How long might the new project last?' Rosen asked himself. His answer was a year, perhaps two. How like our uncertainty, twenty years later, about how long the MSSL group might survive at Holmbury!

Within a week the NRL team had 'electrifying news'; the Army had brought back the V2 rockets from Germany and was setting up a launch facility at White Sands, with the General Electric Company as contractors. What is more the Army was inviting Government Departments and university groups to build instruments to fly in the V2s. Rosen and Krause immediately invited other agencies to join them in formulating an upper-atmosphere research programme. Those responding were the Air Force, the Army Signal Corps, the Applied Physics Laboratory of Johns Hopkins University, Princeton, Harvard, Cal Tech and the University of Michigan. These are all names, with perhaps the exception of the Army Signal Corps, with which we are thoroughly familiar in space research today. If the Army Signal Corps has faded it has left a memento, for it initiated the grenade experiment which Boyd and Groves were to take up with such good effect. From the mixed group of instrumentalists was formed the 'V2 Upper Atmosphere Research Panel', an unofficial volunteer panel which shortly became the Upper Atmosphere Rocket Research Panel. It was this panel that came to Oxford in August 1953.

Rosen and a colleague C.H. Smith began to find out all they could about rocketry with the aim of building their own rocket. They sought out places in the USA where there was rocket activity,

and at the Jet Propulsion Laboratory at Pasadena found work going on with a scientific basis. Rosen later spent a year at JPL learning his new trade.

Research at NRL was by no means lavishly funded. Rosen in his book has little to say about the funding problems, but of course there were plenty. The NRL suffered from a form of funding which the MSSL has recently come to experience. It received a modest fixed annual sum from the Office of Naval Research, with the remainder of its income earned through contracts. It was a very unstable arrangement which hampered the even development of the rocket activities, and it says much for Krause that he was able to maintain the programme.

The first Army V2 was fired in June 1946 and it carried the Research Panel's instruments. But Rosen and his colleagues were not enthusiastic about the V2s. There was, after all, only a limited number of them. They had to be heavily ballasted for stability during powered flight. Worse, they had no spin stability; once out of the atmosphere they rolled and tumbled. 'You can imagine the dismay of scientists who had worked for months to build a solar spectrograph which then only got momentary glimpses of the Sun,' wrote Rosen. I still recall the dismay of Boyd and Willmore when they learned in the late 1950s that theirs was to be precisely the same prospect for many more years with Skylark!

Rosen and Smith at NRL therefore set about designing and building their own rocket, one which, from the outset would be a research tool and which could be pointed if not at the stars at least at the Sun. In fact they designed two rockets, for Smith and Rosen decided to follow two different approaches. Smith wanted a smaller, simpler, more conservative design than the V2; he ended up with the Aerobee, the workhorse of upper atmosphere research in the USA. It was built by the Aerojet General Corporation and was eventually equipped first with a sun-pointing system and later with a gyroscopically-controlled jet system. Rosen wanted a large steerable rocket, very much influenced in its design by the V2 on the one hand and Dr R.H. Goddard's work of the 1920s on the other; he ended up with the Viking rocket.

Having decided on a specification for his larger rocket, Rosen went out to tender, finally settling on the Glenn L. Martin Company of Maryland, close by Washington. As there was no rocket engine available one had to be designed and built, and for this task NRL chose a struggling firm, Reaction Motors Inc., set up during the war by a few amateurs from the American Rocket Society.

The first Viking was fired from White Sands in May 1949. In May 1954 the 11th Viking reached 158 miles altitude, the highest to that time. I particularly recall the beautiful photographs from that flight which reached RAE many months later, with the first intimations of what would be possible in the field of remote sensing.

The Viking and Aerobee rockets plus a solid-propellant third stage formed the basis of a proposal for the NRL Vanguard satellite launcher which was to contribute to the US effort in the IGY by putting the first satellite into orbit. The choice of Rosen's Vanguard proposal was made in the face of a strong counter proposal by von Braun for the Army 'Orbiter' project. When the decision was announced in September 1955, the Army was incensed, one general calling it a 'boon-doggle'! At NRL the staff were elated and surprised, for they had seen their chances as minimal set against the Army's Orbiter with its Redstone rocket, already four years in development. The delays and difficulties which dogged the Vanguard project are now history. The launch of Sputnik 1 on 4 October 1957 occurred whilst Vanguard was in its early proving trials. The first attempt at launching a satellite into orbit, if only a test model, was an ignominious failure. At that, the Army were brought in and on 31 January 1958 a Redstone Jupiter rocket put Explorer 1 into orbit.

This then was the background to the start of upper atmosphere research in the United Kingdom. Fired with enthusiasm after the visit of the Americans, and buoyed up with the knowledge that there was a firm plan to build an upper atmosphere research rocket at the RAE, Massey and Boyd laid their plans for a space research group at University College London

THE START OF SPACE SCIENCE AT UCL

RESEARCH USING SKYLARK

With the beginning of the Skylark programme Boyd began to build up his own rocket group by recruiting scientific staff, technical staff and more research students. Peter Willmore joined the College staff quite early on, coming from AERE Harwell. Ron Stebbings was another staff member, working with Willmore on the early rocket instruments, as was Doug Heddle, who took an early interest in the possibility of making stellar ultraviolet measurements; it was clear, though, that without a head stabilisation system on the rocket the chances of a successful UV measurement would be poor.

As the need for a dedicated mechanical engineering design office and workshop grew, Boyd recruited Peter Bowen, newly released from his national service with the Royal Air Force. Later, as satellite projects began to impose ever greater demands, Tom Patrick and Peter Sheather came from industry, from de Havilland Ltd, bringing with them valuable expertise from the aerospace field. Jim Bowles was recruited from Imperial College; finding electronics development as much to his taste as using the completed instruments, he eventually came to head the electronics design office and workshop.

It is difficult now to imagine a world without transistors, semiconductor devices and microcircuits, but, remarkably, the first transistors, - temperature-sensitive germanium devices - began arriving in Britain from the USA only in 1957-8. Until then, small, high-reliability radio valves were employed, sunk in alloy blocks for heat dissipation. Finding enough battery power for instruments using these valves was a perennial problem. Nevertheless, it was in that context that the development of rocket instrumentation had to begin

Boyd had several research students who eventually went on to success elsewhere, Norman Twiddy, who became a colleague of Beynon at Aberystwyth, John Thompson, whom we were to meet much later in a senior position at Marconi Space and Defence Systems, Ken Pounds, who was to create a successful space research group at Leicester, and Alex Boksenberg, later to become director of the Royal Greenwich Observatory. Len Culhane and Peter Sanford came from Dublin and Leicester as post doctoral fellows. The Department had been blessed in the mid-1950s with a new wing, the first new post-war College building, I believe. But a large expansion was under way in the nuclear physics group and pressure for space began to grow again within the Department, particularly for workshop facilities. Eventually Boyd found refuge for his technicians in the basement of the old Seamen's Institute next door, and there they grew and prospered, both the mechanicals and the electricals, until the time came for the move to Holmbury.

In the earliest days Boyd saw to it that his rocket experiments required the minimum of electronics. Acoustic and photographic observations at ground level of grenades ejected at intervals along a twilight trajectory led to measurements of the upper-atmosphere densities and winds. An inflatable sphere ejected from the rocket and brightly lit in the last rays of sunlight was to be photographed as it fell to yield further information on densities.

The grenade experiment proved most successful. and from 1965 was developed independently by Groves and later by Rees into a powerful research technique. The development of the

grenades, and the bay from which to launch them, was undertaken at RAE, leaving the UCL group, which was led more and more by Groves, to develop the ground instrumentation. We at RAE put the technicalities of the grenade's explosive flash-and-bang mixture into the hands of Ministry experts who had no hesitation in dismissing Boyd's choice of a standard RAF photoflash for use as a grenade. 'Not enough oxygen to burn up there,' said one, 'and the bang won't be loud enough to be picked up on the ground. We'll give you a special mix with an explosive core for the bang and around it the right kind of flash mixture.' 'But you don't need extra explosives,' said Groves, 'if you do the sums you will find out why.' 'Photoflashes will work perfectly well,' said Boyd, 'there's no reason to change the mix.' But there it was; we were in the hands of the explosives experts. They produced a series of grenades. We tested them as best we could at the Larkhill range on Salisbury Plain, and eventually flew them at Woomera. They were a sad disappointment. For a later launch we were able to persuade the experts to make up a mixed batch of grenades, some with standard photoflash fillings, some with their chosen fillings. The photoflash won hands down. Science and UCL had triumphed!

The Army authorities proved extremely helpful when Boyd and Groves suggested that they might provide the sound-ranging microphones needed at Woomera. A visit to the School of Artillery at Larkhill proved most profitable. The gunners were equally amenable to the suggestion that Groves and his team might be given facilities on the Shoeburyness firing range to set up microphones and ballistic cameras (fixed wide-angle cameras with remotely controlled shutters) and to test them against exploding high-altitude anti-aircraft shells. This seemed a most original idea to me, but I later learned from Murgatroyd, then a member of the Meteorological Office group at RAE, that at the end of the war he had been carrying out the same type of measurement.

Bowen had been busy devising a flash detection system, to be carried aboard the rocket and linked on a special channel to the ground, which would enable camera shutters there to be opened briefly in time to record the light of the burning photoflash against the day sky. He hoped to test his shutters at Shoeburyness using a ground-based flash detector. It was, I recall, a very marginal set-up. Having been active behind the scenes in obtaining War Office assistance, I was invited to observe tests at the range. Early one afternoon in July 1956 I found Groves comfortably installed at his control centre, his microphones laid out down range, his pen recorders around him, his team busy at their duties. An Army communications set linked him to a distant gun team, where a crisp young gunnery officer readied the team for the next shot. Gerry was determined to get his sums done after the last one, and as he sat hunched over his papers, slide rule in hand, the afternoon hours ticked quietly away. From time to time his peace was broken by the crackle of the loudspeaker. 'Gun team here. Are you ready for the next shot?' Gerry remained impassive. 'I say again, are you ready for the next shot?' Still no response from Gerry. Time drifted by. The sea sparkled beyond the sand dunes. Then Gerry stirred himself. A decision had been reached. He leaned towards the transmitter, depressed the switch. 'I say, can I have another shot please?' Back came the crispest reply of the afternoon, 'Sorry, the gun crew went off duty half an hour ago'.

At Woomera the range staff did UCL proud. The requirement for ballistic cameras, for which so much preparation had been made at College, merged very comfortably with the range's own interests, and large and very superior cameras were eventually at UCL's disposal. The microphones were laid out in an array which stretched many kilometres across the range, each installed in a carefully surveyed and positioned pit, with its special cover to reduce wind noise. The recorders were installed in the Instrumentation Building, their operation soon becoming a range responsibility. Some initial unreliability of the grenades was traced to their delay fuses being unpressurised. Groves, from his fastness at Gower Street, won WRE respect when he reported, from an analysis of the recorder traces and camera plates after the first successful

experiment, an apparent error in the surveyed position of one of the microphones. The Australian Army surveyors who had put them in were sceptical, but Groves proved to be right.

The UCL falling-sphere experiment was not a success. It relied on the same ballistic camera technique as did the grenade experiment, with coded shutter operations for timing. Bowen and his team spent a lot of time producing the inflatable balloon package, but there must have been problems with its deployment. After a number of abortive trials the experiment was abandoned. It was to be many years and many failures with separable heads and recovery parachutes before it was realised that once outside the effective atmosphere the rocket, which for many years was not spun up to give it stability, gyrated far more wildly than suspected, sometimes even throwing a parachute out of its bay prematurely. It is just possible, I suppose, that something on these lines caused the balloon failures.

Lack of spin stabilisation came to be the Achilles heel of early Skylarks, and none were more aware of it than the UCL group, particularly when they came to plan solar X-ray and ultraviolet measurements. Try as they might they could not persuade Hazell to aim to spin the rocket. Indeed, so concerned was he about the destructive effects of roll-yaw resonance, which had resulted in the first in-flight failure in 1959, that thereafter he took all steps possible to obtain the very minimum spin possible, even after the introduction of the Cuckoo boost motor. The fin manufacturers were made to overhaul their jigs to remove any permanent sets in construction. Fins were each measured individually, and groups of three chosen before assembly in order to produce the minimum overall misalignments. Stronger fins went into production to reduce in-flight distortion as they heated up on the ascent. It was not until production of the Skylark was contracted to BAC (Filton), and ESRO engineers took a fresh look at the aerodynamics when they adopted Skylark, that spin-up at launch and canted fins were introduced, with hugely beneficial results.

The first flight of an operational university payload took place at Woomera on 13 November 1957 with Skylark 4 which was used to carry, amongst other equipment, the UCL grenade experiment. The last flight, of Skylark 1305, took place on 12 May 1978; it carried a solar-ultraviolet spectrometer built jointly by that same UCL group but now at the MSSL, and the Appleton Laboratory, Slough, now part of the Rutherford-Appleton Laboratory of the Science and Engineering Research Council. The serial number 1305 did not imply a number of Skylark firings; the number became a code in the course of the programme indicating the year the project was initiated and the configuration of the payload.

Between 1957 and 1978, instruments developed and built by Boyd's team were carried on 99 of the 200 or so Skylark rockets flown from Woomera as part of the space research programme and on 8 Skylarks flown from Andoya (Norway). Many instruments were quite simple ionospheric probes which could be added to other group's payloads at little cost and effort. Other instruments were used in studies of the Sun, stellar X-ray sources and the magnetosphere. The construction, testing and preparation of these rocket instruments formed the backbone of the group's work for some twenty years, from the mid-1950s to the mid-1970s. At that time a newly-arrived research student might be assigned to a rocket project, and might expect to make it his own, supported by the electronics and mechanical engineering groups. Then, given a fair wind, he might complete the project within 3 or 4 years.

The Skylark rocket itself was transformed over the years from the simple single-stage low-performance (140 km) version of 1957 to the three-stage (787 km) version flown from Andoya in October 1977. The Australian Weapons Research Establishment (WRE) at Salisbury, South Australia, and its Woomera range provided the principal Skylark payload preparation and launch facilities. Many UCL staff, in company with colleagues from other universities, from Government laboratories and from industry, became very familiar with the Woomera

experience; the journey to Adelaide, first with RAF Transport Command, later by charter and commercial flights; the visits to WRE at Salisbury; the early-morning flights to Woomera and the long drives to and from the range head; inspecting the huge but very serviceable launching tower; working in the Skylark test shop; preparing for the oft-repeated launch attempts. After the rocket flight itself, there followed the first urgent examination of the quick-look telemetry records and sometimes participation in the recovery of the payload by helicopter.

COOPERATION WITH NASA

COSPAR, the International Committee on Space Research, had first been convened in London in November 1958. At its next meeting, in the Hague in March 1959, the US delegate announced that the six-month-old National Aeronautics and Space Administration had been authorised to launch without charge scientific equipment for other countries. NASA proposed to use for satellite launches the 4-stage solid-propellant Scout launcher then under development. The British National Committee for Space Research, which first met in March 1959, quickly set up working groups to formulate a list of possible experiments to be flown in satellites. Government funding was agreed and announced by the Prime Minister in May, and in late June a team lead by Massey, and including amongst others Boyd, Sayers and Robins, visited the USA to discuss with NASA what might be done. They came away with an agreement between Massey and the Deputy Director of NASA, Hugh Dryden, formerly Director of NACA, for the launching on Scout rockets at intervals of a year of three satellites containing British experiments. In the first, NASA would provide the structure, telemetry and power supplies; thereafter the UK would progressively take over the satellite engineering. This was a remarkable offer and Sir Harrie would later remark how much it was due to Hugh Dryden and the regard he had for this country. We were to meet others in NASA who had worked with UK scientists on wartime developments who had the same wish to help.

In December the BNCSR had made its choice of instruments for the first US/UK satellite, known as S-51. The chief aim was to further our knowledge of the ionosphere. Instruments built separately at University College London and at the University of Birmingham were to measure densities and temperatures of electrons, and characteristics, quantities and distributions of ions. They were also to monitor solar X-rays and ultraviolet light in order to correlate changes in the ionosphere with the flare activity of the Sun. The X-ray instrument was to be built in collaboration with the Leicester group to which Ken Pounds had recently moved. A further instrument, to be built by Elliot's group at the Imperial College of Science and Technology, made possible a separate study of the cosmic ray particles that reach Earth from outer space. Boyd and Willmore's proposals for the ionospheric probes were based on their growing experience with probes flown to lower altitudes on rockets, and this experience was to expand in parallel with the satellite work. Between 1959 and 1965, 30 Skylark rockets fired at Woomera alone had carried UCL ionospheric probes ranging from simple fixed-potential devices to the more elaborate ac probe. Similar probes were also flown in French rockets from Hammaguir (North Africa) and later in rockets flown from Sardinia by the European Space Research Organisation, ESRO.

The British proposals were discussed with NASA and formal agreement was reached early in 1960. A joint S-51 working group was set up and a project manager was appointed on each side of the Atlantic, R.C. Baumann at the Goddard Space Flight Center, Robins in this country. He, as Hazell's division head at RAE, had some years earlier been at the heart of the arrangements for the UK rocket programme. Then, with so much to be done in close collaboration with Whitehall on the financing of the emerging space programme, Massey had needed advice and help of someone who knew his way around the corridors of power. He was able in 1959 to call upon the Ministry of Aviation, as Supply had now become, to lend him Robins. That is not to

say that Massey himself wasn't extremely familiar with those corridors, for he was now chairman of the Government's Council for Scientific Policy and Scientific Secretary of the Royal Society, but he couldn't do everything. So Robins joined UCL in 1959.

By 1960 the Radio Research Station at Slough had a new Director, J.A. Ratcliffe, and was shortly to become the Radio and Space Research Station, with wide responsibilities to support the UK space programme. A UK-funded NASA Minitrack station was being set up at Winkfield, beside Windsor Park, run by the RSRS and operated by a contractor. A second satellite payload, S-52, was being discussed within the BNCSR and three new groups were to be introduced to the new skills. Amidst it all, the Skylark programme was mounting in size, and support for it at RAE was coming under increasing criticism and strain.

The whole scientific space programme (as distinct from telecommunications, defence and space technology) was overseen by the Royal Society's BNCSR. With the increase in activity Massey and Robins sought more help from the Ministry of Aviation. In mid-May 1960 I was promoted, welcome news, and told I was to join Robins at UCL - even more welcome news because some three years earlier I had been directed to cut down on the time I was devoting to the university programme and to undertake other responsibilities. Now I was to hand them over and assume the role of UK S-51 Project Coordinator. At the end of August I cleared my desk at RAE and left for UCL, apart from one final visit to Australia in October.

3

ARIEL 1

The first meeting of the S-51 Joint Working Group took place in Washington in March 1960. Instrument designs were to be finalised in October and a launch in late 1961 or early 1962 seemed possible. When I arrived at UCL to join Robins, the development of the instruments was proceeding rapidly at Birmingham, Imperial College, and above all at UCL where five were under construction. Willmore and I shared half of a converted laboratory (E1) as an office, a room which to this day remains the MSSL patch in the Department. In the other half, Connie Duncan, the group's secretary, wielded her typewriter. In the laboratory next door Harry Goddard was at work on the tail end of Boyd's laboratory probe experiments. Boyd, Willmore and Sayers, now with several years of experience of their rocket probes, were very quick to pick up the finer points in the new satellite field, and lost no time in making the most fruitful contacts with the NASA engineers and scientists and winning their respect and support.

At the end of November 1960 the second meeting of the Joint Working Group took place in Washington. Indeed meetings of both the S-51 and the S-52 groups were arranged, as by now the S-52 experimenters had been chosen and were ready for exploratory talks about their project. The Goddard Space Flight Center at Greenbelt was the focus of the meetings, though it was as yet still very much a building site, with bulldozers at work clearing the scrub from a large wooded area. The first two completed buildings had been in use only for three months. The staff had been drawn from other Government laboratories, notably from the Naval Research Laboratory, sited at a Naval Air Station beside the Potomac. It was there that some of us held our first meetings. As we entered the gates of NRL, Graham Smith, S-52 experimenter and radio astronomer, pointed out the white parabolic reflector used by the late Karl Jansky, whose discoveries in 1932 were the beginning of radio astronomy.

I had taken with me a pocket tape recorder; with so much to be learned, this seemed a valuable aid. Back in England I was amused to discover how much extraneous noise it had picked up, the naval recruits being paraded outside, the shouts of the instructors, the bugle calls. I also discovered how tedious and time-consuming it is to play back recordings of meetings, and what little of value is extracted from them!

Earlier on that first day we had called on the UK Scientific Mission, to make ourselves known and to receive our dollars. UK currency restrictions were still firm and US money could not be taken abroad. Instead we were issued with our allowances by the UKSM scientific attaché, Harry Bourne, and his staff, who also booked our flights, hotel rooms and daily transport. We were well looked after, with that air of bustling good-heartedness that characterised so many of our contacts in and around Washington. This arrangement continued for several years, during which time the Mission moved from its offices on 19th and K into the British Embassy.

When the S-51 meeting began, under the forceful chairmanship of Bob Baumann, formerly of the NRL Vanguard project, we began to get a taste of the size of the task being undertaken for us at Goddard. We were told of the agonising re-appraisals that were in hand there, and much concern was expressed at what we British were asking of the Goddard team, but they were by and large extremely cooperative and at the end of the meetings nothing had been lost. There was no doubt in my mind that the command of their subjects being shown by the S-51 experimenters so impressed their opposite numbers that thereafter they willingly went along with all that was asked. There was to be a quid pro quo, however, which we discovered when the S-52 meeting began - GSFC were to insist that the second satellite should be of the same build as the first.

When I, as UK S-52 project manager, told the meeting how very different our whole concept was, the Director, Mr Goett, and his assistant-director, J. W. Townsend, were brought in to underline their concern. Eventually it was agreed that the S-52 team would go home and re-think their plans. That is all another story, of course, as UCL were to play no part in S-52, but I recall that it was at this meeting with Dr Goett that the UK was first informally invited to propose instruments for the future large satellites already being planned at Goddard.

A topic which ran through discussions in both working groups was how we, the British, planned to handle the data. Goddard had recently launched its first true scientific satellite. Unlike its predecessors, Explorer VII was spilling out data in large quantities. It carried a non-synchronous analogue telemetry system, inherited from a rocket programme, which modulated the width of the spacing between samples to give additional data capacity. This meant that data samples arrived asynchronously and so could not be digitised automatically for subsequent computer analysis. Bob Bourdeau, the project scientist, had a roomful of data-processing assistants laboriously wading through paper listings. He had, he explained, been taken off-guard by the very success of Explorer VII, and was now urging us to pay heed. Goddard would do their bit by providing a new synchronous telemetry system, and, as it did not yet use a pulse-coded modulation system, they would digitise the analogue samples and write the output to magnetic tape. Would we be ready with digital computers for S-51, we were asked, and would we be ready to digitise the data ourselves for S-52?

Now we were taken off guard. We knew of no tape-oriented digital computer anywhere at home. Worse, the number of computers known to us and even notionally available could be counted on the fingers of one hand. They included the primitive Ferranti Mercury at RAE with its punched paper tape input and the computing power of a modern pocket calculator. I mentioned it to Cyrus J. Creveling, then head of Goddard's rather small data-reduction branch. He took down his reference book of computers, leafed through to the very last page and stopped dead. 'But it uses radio valves,' he exclaimed in astonishment. We in the UK were indeed in the Stone Age of computing.

We were intrigued by the novelty of IBM digital magnetic tape, as were Creveling's group who had just taken delivery of their first tape drive. Albert Ferris, later to supplant Creveling and become head of a hugely expanded data-reduction branch, scattered iron filings on a length of the tape, imprinted at the then standard density of 200 bits per inch, stuck on a length of Sellotape, peeled it off and brought it for us to see. 'Look at that low packing density,' he exclaimed with some scorn, 'I reckon we could do ten times as well.' It sounded pure American exaggeration, but by today's standards Ferris was really being quite modest!

Back in the UK, we had a sharp critic and a good friend in Ieuan Maddocks, head of the Trials Group at the Atomic Weapons Research Establishment and a member of the BNCSR. As a critic, it was he who, used to running a well-funded, well-staffed and strongly Government-motivated technological operation, had been outspoken about the shortcomings at RAE on the Skylark programme. As a friend it was he who in July 1961 offered the use of AWRE's newest acquisition, a pair of IBM computers, the 1401 and 7050, with their all-important and still very novel magnetic-tape drives. This offer solved our immediate problems, though pressures at AWRE were soon to cause our work to be shifted to a similar computer installation at Warrington, most inconvenient in the absence of any form of data-transmission system. Towards the end of 1962, with data tapes piling up, even the Warrington facility was to be closed to us, and I began negotiations with the Central Electricity Generating Board who had an identical facility in central London. There, freed from the complications of working in a defence establishment, the UCL group did much of its S-51 computing. In time Imperial College acquired a similar facility which UCL could share.

Space science was by now acquiring a public image. Lecturers, broadcasters, authors were much in demand, and in December 1960 Boyd gave a Royal Institution discourse on 'The New Astronomy'. He and Willmore had by now undertaken a daunting programme, several instruments in Skylark, others in the French Veronique, more in Canadian Black Brants, in the RAE Black Knight re-entry vehicle, and now the S-51 payload. Moves then afoot to create a European Space Research Organisation were also absorbing some of Massey and Boyd's time. A preparatory commission, COPERS, of which Boyd was a member, was set up in Paris to pave the way. At the COSPAR meeting in Florence in April 1961 a further step forward was taken, following moves by COPERS in Stockholm the week before, to set up four sub-groups to formulate proposals for key aspects of a research programme. Now decisions were needed on the membership and exact objectives of the four groups. The decisions were not taken in a smoke-filled room but out in the sunlit courtyard of the Pitti Palace by a relaxed and optimistic group of Europeans including, for the UK, Massey, Boyd, Robins, Ratcliffe and myself. H.E. Newell of NASA HQ, a respected space scientist and policy maker, stood close by, taking a keen interest in the birth of this infant force in the international world of space science.

A key date in the S-51 schedule was 1 June 1961 for first-model delivery. Willmore took the UCL equipment to Goddard and soon settled in for a long stay. Sayers was less well prepared as his contractors had let him down. Whilst at Goddard in February I had been shown round the GSFC's worldwide communications centre with its banks of teleprinters. This centre served the Minitrack system, with one line going to the RSRS station at Winkfield. When I asked what the chances were of having a communications link, known as TWX, into College the reply was immediate, 'Sure - what do you want to call it? How about UNICOL?' And so it was.

The pull that NASA had with the GPO at that time was such that by the time that Peter Willmore was installed in Washington the teleprinter was working in Room E1 at Gower Street, on a party line shared with Winkfield, RSRS and Jodrell Bank. Shortly afterwards our calls were routed through a NASA switching centre in London, and the party-line arrangement ended. We were at once in daily touch with Willmore, telephone calls to the USA being then a costly and carefully circumscribed luxury. Willmore's car sat in the College quadrangle with ever flatter tyres until negotiations with Bowen for its sale commenced, not by letter, nor by telephone but by TWX. Many months later when visiting Goddard's communications centre I was presented with a handful of old UNICOL TWX messages, amongst them the very lengthy Bowen/Willmore negotiations. Unknown to us, there had been a recording angel at work, our misdemeanors were all on record, and now I was being asked to put my foot down with a firm hand!

Thereafter Willmore bore the brunt of the UK burden of work at Goddard, living in an apartment in Washington and driving to and from Goddard in a car provided by the UKSM, a steely grey Ford Falcon which soon became well known to us all. As the prototype satellite was built up, the instruments were installed and by November 1961 the thermal-vacuum tests begun. Al Durney, a research student in the Imperial College group, spent long periods out there too, eventually being joined by his girlfriend, Gerry Smith.

Home for Christmas 1961, Willmore had brought a novel request - could we arrange to set up a telemetry receiving station near Tristan da Cunha in the South Atlantic? He wanted one pass of S-51 data as the satellite went into orbit to verify that booms and solar paddles were deploying in their correct sequence, this being critical to the correct orientation and spin rate of the satellite. Tristan had become uninhabitable in the past few months because of volcanic activity and the inhabitants had been evacuated to Britain. If the job was to be done it would have to be from a naval vessel. Massey, a wartime Head of the Admiralty Mines Establishment, was able to get the ear of the Deputy Controller of the Admiralty, Sir John Carroll. On 24 January I visited the Admiralty to discuss with Sir John's assistant the possible use of the South Atlantic

Protection Vessel HMS Protector. True to form the Admiralty went into immediate action. The very next day Robins and I, with John Smith, who had now joined Robins and myself from AWRE, and Ken Fea from College, were at the Admiralty Surface Weapons Establishment, Portsmouth, to see the Superintendent, Dr Stewart Watson and the Captain Superintendent, Captain Best. Yes it could be done, and yes we were correct in believing that gun turrets these days were stabilised and would provide an excellent pointed mounting for radio aerials. We went aboard HMS Sheffield in Portsmouth Harbour to inspect the twin 4-inch guns, as fitted to HMS Protector, and saw how our receiving aerial could readily be strapped between the barrels. We met Frost from ASWE who would be lent to us by the Director, and who would team up with Fea to see the operation through. Goddard, ever cooperative, ever ready to help those who help themselves, found us the receiving equipment. Arrangements were made to link HMS Protector through to the launch site via the NASA station on Ascension Island for the countdown. In mid-March 1962 a final briefing meeting at the Admiralty cleared the way for the operation and Frost and Fea set off for the Simonstown naval base in South Africa. There they and their gear would be picked up by HMS Protector before its week-long journey to Tristan da Cunha.

Changes had taken place in NASA's launch plans. The development of the Scout launcher was well behind schedule and so, at its own cost, NASA had decided to provide a launch on a Delta rocket, at Cape Canaveral rather than Wallops Island. This was excellent news, for Delta had good performance and reliability, but it meant a new installation under the nose cone and an extra mounting structure on the Delta's solid-propellant third stage motor. That in turn meant changes to the vibration specification; the new mounting was acting like a spring and exacerbating the vibration environment. Willmore wasn't happy with the quality of the work on the wiring harness in the spacecraft. He asked the redoubtable Bob Baumann that the job be re-engineered - and won the day. Changes were also taking place in Peter's domestic life, for Gerry Smith had moved in with him, leaving a disconsolate Al Durney.

Preparations for the data-handling task in the UK were helped when some of my former colleagues at RAE decided it would be of interest to become involved, and they assigned a task to Win Lloyd. She took it up with much enthusiasm, and managed to persuade her department to send her out to Goddard for a couple of weeks in March. Indeed she was there for Peter and Gerry's wedding reception. But Win wasn't much the wiser for her visit. She found the experimenters far too engrossed in the practicalities of instrument preparation to find time for her, and she came back rather depressed. Today, after years of hard experience, the lessons which she and everyone else connected with those early projects began to learn then have changed the whole approach to the design of an experiment and the way plans to handle the data are laid.

Week by week S-51 came together and in March the flight model was shipped to Cape Canaveral for a launch attempt on 10 April 1962. Peter Willmore (with Gerry), Peter Bowen, Keith Norman, John Wager from Birmingham and Alistair Durney went with it, in time to see the rather depressing spectacle of a missile being destroyed just after launch, showering lumps of burning solid propellant around the S-51 launch pad. Robins and I arrived at Cocoa Beach on the 6th, Sayers and Elliot, Durney's group leader, the next day, and Massey and Boyd on Sunday 8th April. HMS Protector came on station that day, and we listened to the communications tests, first the link-up to Ascension Island and then the ship reporting that all equipment was working, everything in readiness. The satellite itself was cocooned atop the Delta rocket in a canvas shelter with warm dry air blowing around it. The whole payload was behaving impeccably and most of the NASA and British payload teams were relaxing down at the beach.

On the Monday a film crew arrived and besieged us. Goddard was making a documentary film of the first US/UK satellite project and here was an opportunity for them to get certain key performers together for a few background shots. The performers complained loudly, but the truth was that most of them, Massey included, were today the extras, the principals being closeted in the telemetry trailer doing the real stuff. So we were filmed, and for good measure the film director sat us around a conference table and had us pretend to be at the Royal Society meeting where the experiments were first selected! In the afternoon we British visitors were taken on a tour of the Canaveral complex, at one point being called out to see an Atlas ballistic missile launch. We could just make out the tower on the skyline. The countdown came over the loudspeakers - 5, 4, 3, 2, 1, zero. A huge hemisphere of white smoke appeared. We paused, expecting to see the rocket shoot skywards through it, but then the truth dawned - the Atlas had blown itself up on the pad!

At midnight the countdown commenced, for a 1pm launch, and all went smoothly. By 9 am on a beautiful hot day the gantry was drawn back and we were able to walk out and admire the Delta rocket in its gleaming whiteness, the crossed Stars and Stripes and Union Flag on its side. NASA marked this notable international first by flying a Boeing 707 down from New York to Cape Canaveral with the United Nations' 'Peaceful Use of Outer Space' committee aboard. The spare S-51 payload was on display aboard a truck and the committee clustered around it. The rest of us stood about and enjoyed the almost carnival air, with perhaps 150 people there in the sunshine. A plume of liquid-oxygen vapour drifted lazily away from the Delta rocket, reminding us of what shortly was to happen.

When the launch pad was cleared, the rocket was given its last load of liquid oxygen, and at 12.25 the final countdown began. At minus 6 minutes a hold was called. Then, as we watched the TV monitors, the rocket was enveloped in clouds of white vapour - the oxygen was being dumped. A fault had been detected in the second stage. It later transpired that the compressed-helium sphere immersed in the second-stage oxidiser was leaking and pressurising the tank much above normal. The casing was within minutes of rupturing and deluging the first stage with acid. The launch was off for a couple of weeks.

With Easter barely ten days away and home calling, most of the British visitors returned, though I preferred to take a holiday with friends at St Petersburg Beach on the Gulf coast and then go on to Washington for more S-52 business. HMS Protector couldn't stay for the second attempt and had turned about and set off for Simonstown. The Admiralty assigned the frigate HMS Jaguar in her place. With a week's voyage each way there was no time to spare. As Protector berthed alongside Jaguar, down went the gangway, up came her men. The boxes of equipment were hoisted on their backs and carried aboard the frigate, followed by Fea and Frost, and soon they were once more at sea.

At Cocoa Beach all was quiet. The experimenters were back from a tour of the Everglades. Some had moved out of their motel rooms and hired apartments beside the sea at Winslow Beach, and I followed suit. The weather was fine and hot, the sea warm, the beach perfect, with miles of clean sands shelving gently down, making for ideal bathing in the Atlantic rollers. How much more invigorating than the lazy Gulf! At the Cape there was little for the experimenters to do. On Tuesday 24 April the British party were all back again.

Activity elsewhere on the huge Canaveral range was intense, though sometimes unprofitable. An errant Pershing rocket was cut down at midday; a Minuteman was blown up after dark, in a brilliant display of pyrotechnics. Debris fell to the sea, bounced, and lit up the horizon as it burned. A Ranger moon probe on an Atlas-Agena launcher got away beautifully. The probe hit its target three days later, but all to no purpose - the main programmer had failed at launch. On April 25th the first stage of the Saturn rocket was tested - a million and a half tons of thrust and

a payload of ninety-five tons of water, deliberately blown apart 50 miles up. Massey, Boyd and I stood on the beach and watched the vast, spherical cloud of water vapour form instantaneously, then fade rapidly. There was nothing more to be seen. 'They should have put a railway locomotive into orbit, not water,' said Cy Creveling, 'Now that would have impressed the world.' Amidst all this, our faith in the Delta rocket was solid. A failure tomorrow was unthinkable.

The countdown began at midnight, most of us taking a nap beforehand. Car drivers went round the motels picking up their passengers. My two were at the Polaris, one being Al Durney. When I got there he was nowhere to be found until I peered into the faint blue light of the bar. Two men gazed mournfully into their beer, and four ladies of pleasure, who plied their trade on Cocoa Beach, chatted in a corner. On an otherwise deserted dance floor was Durney, doing the Twist to the raucous music of a 4-piece band.

The payload checks went quickly and we were soon back in our apartments to snatch a little more sleep. At 10 am, April 26th, the scene on the launch pad was as animated as before. At 12.30 the countdown was resumed, the firing button was pushed - and nothing happened. A wiring fault was found in the sequencer - nothing irretrievable. At 13.01 the Delta was away, a beautiful sight against the flawless blue sky. After a few minutes Antigua reported the third-stage firing. Forty minutes later Jaguar came through reporting twelve minutes of good signals. At the Cape a teleprinter operator had been put at my disposal. Though aware how little there was to report, I sent off a string of messages to John Smith, standing by in London. At 14.40 the satellite's telemetry was picked up at the Cape, the signals being of such quality that the experimenters were soon looking at good data. One instrument was at once seen to have failed to survive the launch; Jim Bowles's Lyman-alpha detector was dead. The sequence of yo-yo despin, boom and solar paddle deployment and final separation, which Willmore had been so anxious to have observed from Tristan da Cunha, seemed to have gone to order, though there was the odd observation that the orientation of the satellite's spin axis was wrong. It was not for some months, the time taken for the South Atlantic tape to be recovered, shipped to Goddard, digitised, returned to the UK and analysed, that Willmore pieced together the true sequence of events over Tristan. The yo-yo despin device had released correctly, but the cords that held down the booms and paddles had not been adequately protected from the heat of the final stage motor case, around which they were wrapped. Instead of being released in correct sequence by a series of timed explosive cutters, the cords had burned through erratically. Thus unbalanced, satellite and last stage motor had coned around the correct spin direction before the release mechanism separated them. The final spin rate was right but the spin direction wrong. Nothing else seemed to have suffered.

One question that had been very much on Massey's mind two weeks earlier had been how best to show the NASA team his appreciation once there had been a successful launch. Possibilities for some kind of celebration were much improved by the arrival, all the way by car from Washington, of a British Embassy representative bringing Sir Harrie the Ambassador's good wishes for a successful launch and a carton of twelve bottles of assorted spirits. Bowen was then charged with the task of finding a venue for a celebratory party. He located the Cocoa Beach village hall, but alas the local byelaws forbade the importation of alcohol. Whilst uncertainty reigned the cancellation had taken place and, as we began to leave, the question arose - what to do with the bottles?. Answer - give them to the UK Project Coordinator to deal with, who else?

Next morning I called on Group Captain Dadswell, RAF liaison officer at the nearby Patrick Air Force Base, with a problem - or rather with two. Where could I stash the bottles, and where might Sir Harrie in due course have his party? 'No problem,' said the good man, 'put the bottles in my safe, and leave it to me to book the Officers' Club here at Patrick - it will be perfect. Just tell Sir Harrie to give me a ring when the party is on and I will fix it with the mess secretary.' It

was all so easy. Two weeks later as the countdown proceeded, Bowen consulted Massey as to whom he should invite. 'Everyone,' was the reply, incautious but understandable amidst the euphoria, and Bowen chalked a notice on a large blackboard. 'In the event of a successful launch, the British invite everyone to a party at the Patrick Air Force Base at 8pm.' The word went round like wildfire. Cocoa Beach was a small place and there wasn't that much social life. Wives down for a short holiday whilst their husbands worked were ready for a spree. At 8 pm I delivered the 12 miserable bottles of spirits to the club, and was shown the room where the party was to take place, a fine, spacious, airy place with two bars staffed by white-coated mess waiters. Half an hour later the guests came flooding in. 'Everyone' had been interpreted literally. The Delta contractors from Douglas Aircraft who had their own operations building, had heard about the party and they came too, with their wives. Soon the room was packed. The mess secretary found me, not Bowen. 'Alright to open up the other room, sir?' 'But there will be nothing for them to drink.' 'Oh, we have plenty in the cellars, sir.' And they rolled back the sliding doors to their full width, revealing another huge room, with bars at each corner ready for yet more white-coated mess waiters. The guests advanced into the newly-opened acres. No stopping events now. 'After all,' I told myself, 'it's Sir Harrie's party; he must answer for this.'

There was an 05.30 NASA plane back to Washington on the Friday morning, a press conference at NASA HQ, and then later in the day cocktails at the British Embassy. The launch had been announced in London by the Prime Minister, Harold Macmillan, and he named the satellite Ariel 1. He was advised on that by Quintin Hogg, then Minister of State for Science, from his office in Richmond Terrace.

On Saturday morning our good friend and ally Cyrus Creveling was at the hotel with his camper van. Cy was an Anglophile. As a young US Navy technical officer during World War II he had been shipped to England to work at the Radar Research Laboratory, Malvern, on the newly developed centimetric radar. He hadn't forgotten his friends the British. Now he was here to take Boyd, Willmore, Wager and me to his Litton site where he proudly presented the first processed data, neatly demultiplexed and listed experiment by experiment, as promised. With stacks of computer printout under our arms we left for Goddard, stopping en route for a hamburger lunch. The sheets were laid out on Cy's camper table. The first analysis began, with slide rules, pocket calculators being years away. The contrast between Thursday's tensions at Cape Canaveral and Saturday's relaxed lunch at a Maryland hamburger joint was delicious. Boyd and Willmore were evidently very pleased. Wager was not so sure, and when we ran into Sayers a little later he confirmed that all was not as it should have been. The Birmingham probe was being driven in potential so far from the expected level that even its own voltage sweep was at times proving inadequate. Were the various probe sweeps in conflict? Time would tell a happier story.

A COSPAR meeting opened in Washington next day, and there was the newly-named Ariel 1 prototype on display, its gold plated exterior shining proudly. On Monday the S-52 team assembled for their next joint working group meetings at Goddard. On Thursday Willmore and Pounds reported to COSPAR on their first measurements. At another session two astronauts, Titov from the USSR and Glenn from the USA, talked of their experiences. The session chairman, the astrophysicist van der Hulst, appeared with a pair of wooden Dutch clogs. In a gracious speech he presented one clog to Titov, the other to Glenn, expressing the hope that one day they would be brought together again.

By early June the first magnetic tapes were on their way to the UK and by July they were flooding in. A dark cloud had appeared on the horizon however. News had leaked out of the US intention to begin atmospheric tests of nuclear weapons; a hydrogen bomb was to be detonated in the radiation belts to see what effect it might have, and Sir Bernard Lovell was issuing dire warnings. At UCL Boyd and Willmore had discussed how Ariel 1 might be well placed to

observe the results, but when, on July 9th, the 'Starfish' bomb exploded over Johnson Island there was more than interesting measurements to think about. An intense though short-lived artificial belt of high-energy electrons had been created below the natural belts and Ariel 1 was in the thick of it.

The first we heard of these misfortunes was a call from Baumann to say only that on July 13th the satellite went off the air. By great foresight a battery protection device had been installed which detected a low voltage and closed down the payload for a period to re-charge the battery. Only the transmitter remained alive to keep the tracking network in touch. Everything pointed to radiation damage of the solar cells. Thereafter the satellite went on and off as it conserved its dwindling resources.

Towards the end of July, printouts sent over from Goddard revealed that there had been very high count-rates in the UCL/Leicester X-ray detector and in Quenby's Imperial College cosmic-ray detector, beginning within minutes of the burst. Quenby saw the counts increasing with time, not decreasing as NASA experts had predicted. On July 27th a message went from UCL to Goddard to report the Ariel 1 experimenters' suspicion that the solar cells had been damaged. This triggered an angry denial from the US military, but when two US satellites fell sick they began to think again and a conference was called. Quenby and Boyd flew to the States for a meeting over an August weekend, but even there the exact cause of the power failure was not certain. On their return they reported that the fault could still be something internal rather than radiation damage to the solar cells.

Not until the launch of a Discoverer satellite instrumented to measure the radiation environment was the true picture revealed. An intense belt of artificial radiation had been created. The lower-altitude electrons had decayed within a day, but a higher belt would persist for a very long time. The Ariel 1 solar cells were of a type particularly prone to electron damage. A rapid and expensive switch to a type less vulnerable would have to be made by NASA for its future satellites.

Meanwhile Ariel 1 limped along, its tape recorder out of action - a bad blow to Sayers and Quenby. Performance was best during the periodic all-sun phases which lasted a few days. Willmore, in a fit of depression, reported to the British Association in September that Ariel 1 was, to all intents and purposes, dead. That didn't go down well within the project! Much discussion went on about the possibility of the RSRS posting a telemetry receiving station at its station in the Falklands to cover in real time the region of the South Atlantic magnetic anomaly, but it led to nothing.

Ariel 1 continued to survive for a year or more, though with its instruments going out one by one. A modus operandi had been established which enabled data to be gathered at a rate perhaps a third of that achieved before the explosion. But, as a higher-than-planned orbit had resulted from the Delta launch, the satellite was giving three times the planned coverage so things were, in one sense, all square.

In December 1962 Robins, Smith and I moved out of College to our own offices at 3 Chester Gate, Regent's Park. Now firmly under the wing of the Office of the Minister for Science, we were to be known as the Space Research Management Unit, with more staff and space. We eventually found room to house the growing bulk of Ariel 1 magnetic tapes, and I found some time in which to help in the onerous preliminary processing of the UCL data at the CEGB centre. A growing UCL data-analysis group was being built up at College, amongst them John Raitt and Lyn Henderson.

Looking back, one can see that the UCL instruments that went aloft on Ariel 1 on 26 April 1962 were defining the directions that research at MSSL would take over the next 20 years. The ionospheric studies led to a thriving programme of space-plasma physics while the solar instruments led to an intensive series of investigations using first the NASA OSO spacecraft and then, more recently, NASA's Solar Maximum Mission, SMM. Most interesting of all, the development of X-ray counters at UCL and Leicester paved the way for the entry of both groups into the soon-to-be-discovered field of X-ray astronomy.

From my diary:

“January 25th 1963. Up, and to the Office. At noon to Burlington House where came much company, amongst them my Lord Fleck, Sir H. Massey, Sir H. Florey, Sir P. Linstead, all to honour two officers of Her Majesty's ship Jaguar. By and by to luncheon, which was very good and plentiful, when Sir H. Florey did present to Commander Goodhugh a plaque whereon was inscribed the thanks of the Royal Society. This did cover the good work in carrying to Tristan da Cunha our telemetry receivers, and Mr Goss and Mr LeMaître, two geologists who were mighty interested in the volcano which caused the inhabitants of that remote and barren island to leave. They, alas, do now intend and are quite set upon their return and none can dissuade them.”

4

THE MOVE TO HOLMBURY ST MARY

FINDING A HOME

The successful entry of Boyd's group at UCL into the new field of satellite research between 1960 and 1962 led to a steady growth in staff numbers. Accommodation at the College was soon inadequate and Boyd began to look out for premises into which his group might expand, but where were the necessary funds to be found? Help came from Dr F.E. Jones, a former Deputy-Director at RAE and a major force in establishing the liaison that led to the Skylark rocket programme. He it was who, with Massey, formally announced the high altitude research programme in a letter to *Nature*, published on April 7th 1956. Now, as technical director, later Managing Director, of Mullard Ltd, he was able to help. His firm offered to donate funds to UCL for the purchase of premises which could be converted into a space science laboratory.

Boyd found the choice of premises during a short-lived period of prosperity in the first half of the 1960s not easy to make. He had scoured the south of England in search of somewhere suitable close to the two main airports, not too far from his and Massey's homes south of London. Eventually he found Holmbury House. Its previous owners included the Hon. A.E. Guinness who lived there in the 1930s and 1940s and who had a great enthusiasm for modern gadgets; he had filled the house with them. He also piloted his own autogiro from the field below the house. After his death in 1946 the estate was briefly owned by the young Maharajah of Baroda, a racing driver, and his film-actress wife. She ran away, taking with her jewellery which he had bestowed upon her, and he sued her for it through the courts in an action which became a cause célèbre.

HOLMBURY HOUSE

Holmbury House is situated on a hillside above the village of Holmbury St Mary, near Dorking in Surrey. It is a Victorian mansion, standing in 30 acres of unspoiled country, in an area of great natural beauty on the southern slopes of the Surrey Hills, midway between Guildford and Dorking. From its high position the mansion looks southwards across twenty miles of beautiful Sussex countryside to the South Downs, immediately beyond which lies the English Channel, said to be discernible in the right light from the top of Holmbury Hill. When the house was built in the middle of the nineteenth century it was well staffed with servants and a cohort of gardeners. Water reservoirs, a large kitchen garden, extensive greenhouses, a huge conservatory, an underground cold-storage area for food, and, later, a self-contained 110-volt generating system, all now abandoned or totally removed, made daily life as self-sufficient and as comfortable as possible on the then remote Holmbury Hill. The estate comprised much more than today's modest 30 acres, taking in larger tracts of hillside above and extensive farmlands below. The hill road from the village ended at the imposing entrance, where, through large iron gates supported by pillars and flanked with walls of soft limestone, one glimpsed Holmbury House set at the end of a long drive amidst carefully landscaped gardens and grounds. Just inside was a gatehouse; another of similarly modest design guarded a second gateway to the narrow road which runs on down the hillside from Peaslake to Ewhurst. Travellers wishing to make their way across might, if sufficiently determined and self-assured, ask permission of the gatekeeper to walk through, as was recounted by the author of 'Surrey Walks' at the end of last century. He, then a Member of Parliament, tells how he made his way up Holmbury Hill,

shaking off the dust of Felday with relief, for, he said, he had never come across so miserable a village, except perhaps in Bengal! In fact the very railway which had brought him down from London to engage in his Surrey walks was shortly to transform Felday, bringing a new church, big new houses, a re-built village and a new name, Holmbury St Mary. Today the Holmbury House estate is bypassed by what was once its own estate road. The gatehouses have been removed, relatively recently, as have many of the fine trees through old age or the ravages of storms.

To the west end of the property when UCL purchased it was a new house, built by the previous owner, Mr de Havas. He had run at Holmbury a school for handicapped children, which had closed upon his death. The new house had been sited on the former tennis courts whose handsome redbrick pavilion, said to have been a Lutyens design, remains. Further to the west was the swimming pool, built by Guinness for his wife as a birthday present, and rejected, so the story went, because it was not ready on her birthday. The pool and its once comprehensive filtration and chlorination plant were in a deplorable state. The pool had been drained by the school, we were told, and rabbits kept there, but this seemed unlikely as we found it to be serving as a sump for surface water from around the headmaster's house. With this water there had come off the hillside a mound of silt, which had to be cleared away. Frost had penetrated the filtration plant and cracked the pipework. A team of volunteers cleared the pool, piped the surface water around the lip of the pool and away, and restored the damaged plant.

I JOIN UCL

UCL took possession in 1965 at a time when considerable changes were afoot for civil science, for, on 1 April, a major re-organisation was to take place. The Science Research Council came into being, sweeping up the space programme and therefore the SRMU with it, and centring itself at State House in High Holborn. I was to be offered the option of returning to the Ministry of Aviation or of transferring in due course to the SRC. I was loathe to leave the university environment. Even as members of the SRMU several of us retained our honorary appointments at UCL, and were in the habit of lunching there most days. In mid-March 1965 Boyd told me about Holmbury and his plans for the Mullard Space Science Laboratory. Would he have me as a member of his group, I asked. He took it up with Massey. On 31 March 1965 a sherry party was held at Chester Gate to mark the end of the SRMU, and there the deal was struck with Sir Harrie and Robert Boyd and my senior colleagues. I would resign from the civil service and join UCL on July 1st as a member of the lecturing staff, half scientist, half administrator. I had been particularly insistent that I should not have an administrative post; after all I was making the change to get nearer the science. A few years later, when assailed by the College administrators for insisting on taking my orders from the Head of Department and not them, I was glad to be able to call upon that agreement.

CONVERSIONS & ADAPTIONS

In July 1965 I visited Holmbury House for the first time. It was an empty shell, with no water and virtually no electricity. True, a mains cable came into the garage block, but, apart from feeding a single 13-amp socket on the first floor of the main house, its purpose had been to supply a large transformer. The whole building was wired with 115 volts! Perhaps this had suited Guinness; perhaps it meant that the electrical equipment he had brought over from the States, his amplifiers, his huge record player, his film projector and heaven knows what else, would continue to function.

One feature of the house had been its sumptuous bathrooms, each differently furnished in marble, one in white, one in black, one in pink, one in a striking blend of glass and marble. I

found them in ruins. A member of the group had come to Holmbury and smashed up every bathroom in an act of spite and vandalism.

Plans to adapt the main house to our needs were well advanced, and a firm of architects engaged by the College was well at work, though quite how the conversion was to be funded was obscure. Massey had been aided for some years now by Doug Davies, a physicist of rare administrative ability who had given up his scientific work at CERN in Geneva to take over the running of the Department. He, working in close liaison with Boyd and Willmore, with the Bursar's office and with the architect, had gone a long way towards deciding how the building might be modified and put to use as a laboratory. I saw at once that too much emphasis had been put on the provision of staff accommodation. Not only the whole of the second floor had been designated, not only the two flats that remain today but three others, and all had been included in a successful planning application to the local authority. One of my first tasks was to persuade my new colleagues that at least two areas must be returned to laboratory use, and this was done well before the building contractor moved in. In due course rooms on the second floor were recovered and more recently another flat has been converted to laboratory use. Otherwise the allocation of rooms remains much as foreseen, with a surprisingly small amount of structural change being required. The elegant pine-panelled boudoir in the southwest corner on the first floor, for example, is still today, as was planned in 1965, the drawing office.

The original construction of the house had been in some ways surprisingly old-fashioned, some hair-and-plaster dividing walls being found, but the house had been enlarged - the east end for example was an addition, as can be seen from the outside - and in some places rebuilt. As the architect pointed out, when builders are working for a millionaire they usually do not stint on materials. To prove the point he showed me steel girders where timber might well have done - a reassuring thought in view of the heavy loads that we should shortly be imposing on some suspended floors.

The architect found it difficult to get the physicists to define their laboratory needs. A laboratory, he seemed to think, has rows of fixed benches, water taps, fume cupboards; his fingers seemed to be itching to draw them in. But apart from Bowen, an engineer of course, he could get no lead, because physicists generally do not want things to be fixed down. With Bowen he had a soul mate who knew exactly what was wanted - an air-conditioned vibration test facility, complete with an isolation block let into the floor, a control room with a viewing window, a large access door through which to wheel his equipment. This was almost the architect's first commission and he got his teeth into it. The chosen site was the former laundry in the north-east ground-floor corner. This room served us for a while as a temporary dining room, but in due course work went ahead on the conversion, with an air-conditioning system which, I seem to remember, was never effective. The isolation block was abandoned.

The kitchen was another facility where the architect needed no encouragement. He engaged a specialist firm of catering consultants, who produced the stainless steel counters, the British Rail coffee percolators, and much more, all installed in a re-built kitchen area which at that time reached to the rear exit. Later we were to regret this lavish use of space and began to encroach upon it for yet another laboratory.

In the library and main lounge we had, ready-made, our own library and common room, with little to be done except improve the lighting and then to provide appropriate furnishings. The choice was all mine. I had not realised until then that manufacturers only held office furniture in stock. Everything else had to be specified and built to order. The tub chairs in the common room, for example, were made to my specification - not so hard as to be uncomfortable, not so soft as to send people to sleep in a lecture! A prototype was made, I tried it on for shape and tilt, with the manufacturer standing by. With adjustments agreed, the order was placed. Laboratory

benches and tables, library tables, hall display cabinets, were all made, at the furnishing officer's insistence, to my specifications, though my experience of such matters was nil. Only the slightest interest was shown by my new colleagues, and, though this was to me inexplicable, it was also a great relief because there was never any argument!

The workshop was, *faute de mieux*, to be created by the conversion of a row of decaying outhouses on the far side of the high garden wall, which had once supported a large conservatory on its sunny side. At each visit the architect had sighed more deeply. Clearly he was never going to make a silk purse out of a sow's ear. It was damp, there was an open underground drainage system, and, heaven forbid, a reservoir under one room. The wonder is that in course of time he got us as much as he did.

A major source of concern in the main house was the heating system, old and inadequate. The firm of consultants engaged by the architect to deal with all the water supply, plumbing and heating problems produced a cost estimate far exceeding what could be afforded from the notional budget still being negotiated. So the Bursar ordained that there could be no new boilers. We should have to supplement the central heating with electrical storage heaters and otherwise make do with what we'd got - two dismal boilers, one for heating, the other for domestic hot water. They were at the rear of the house below ground level, in a black hole which flooded after rain, sometimes immersing the central-heating boiler and putting it out of action for days.

The College had inherited with the property a gardener and his wife. They were Spaniards who lived in the lower lodge, illicitly housing, we were soon to learn, two relatives in space that would scarcely suit a bachelor. The wife, Mercedes, became our first cleaner. Davies had by the time of my arrival recruited our first two local staff - Ted Pullen, driver, and Mr James Coggins, late Laboratory Superintendent, Makerere University, East Africa. Coggins had recently returned to this country with his wife and three daughters, and they were temporarily housed in the ex-headmaster's house, to the family's general satisfaction. The College's Secretary, A. Tattersall, himself newly returned from Makerere, had known the Coggins family, and this was what had drawn Coggins to apply for the post. He got it, but it never came up to his expectations, for he had been sure that in it he would return to some semblance of the responsible position he formerly held. Having to vacate the comparative luxury of the house in September 1965 to make way for Willmore and his family, and having to make do instead with the stable block, went hard. Even he and his family's subsequent move to the then spacious and newly-converted second-floor flat did not make up for the otherwise unrewarding job. He was being asked to make bricks without straw. Though his wife became our first cook and produced excellent meals, all was to no avail. Eventually he was asked to see the Secretary at College and a parting was arranged.

A FINANCIAL CRISIS

Behind these first months of planning there lay a serious financial crisis. Even before I had left my old job I had sat in on a meeting between the College authorities and the architect to review the latter's estimates for essential and desirable works at Holmbury. Tenders were to be called for in August 1965, and the architect needed to know what money would be available. In fact his estimates were three times the notional sum set aside within the College's overstretched funds for the Holmbury adventure, which, it has to be stressed, was not the outcome of a College initiative but was a departmental activity. The desirable items were heavily cut into, and the meeting broke up with hope being expressed that the University Grants Committee, which was fairly certain to provide equipment and furnishings funds, would also help out with a building grant.

Soon I was launched on the preparation of the furnishings list, a scientific equipment list, and specifications for the telephone installation. By the end of July, agreement was reached with the College that the necessary building money would be found by borrowing, but there was even a difficulty here. If the UGC was to provide a building grant it might not allow it to be supplemented by the College. The UGC had been bitten badly in the course of the recent rash of new University buildings by some being carried out in a style well beyond what they had approved.

Matters came to a head in August in the wake of a national economic crisis and the announcement by the Wilson government of a series of financial restrictions on public spending, amongst which there was to be a cutback on University spending. The Government would allow no new building contracts for six months, yet here was the Assistant Bursar within weeks of letting the Holmbury contracts! The Secretary, Tattersall, and the Finance Secretary, John Tovell, met Massey, Boyd, Willmore, Davies and me. There was more bad news, to me the more appalling because I had not been warned, that a series of building schemes now in train at College were predicated on Boyd's group moving out of its accommodation there. No delay could be countenanced. The group would have to move to Holmbury, conversion or no conversion, and the sooner the better.

Massey was in despair. 'There's nothing for it but to send all the theorists in the Department on long leave to make room for the experimentalists. Of course, most of them will go to the States - I can't think why they haven't gone already. Then we shall just have to run the Department down. It's obvious the game's up and we'll just have to close down.' It was all very hard on Tattersall and Tovell, since the financial squeeze was no fault of theirs, but they also clearly felt that the Physics Department had taken the College for a ride. 'You have tripled your estimates of conversion costs, doubled your recurrent costs. The College can't be expected to find money on this scale.' Massey and Tattersall sat opposite each other, flushed and angry, gazing fiercely at the table top, screwing their fists in embarrassment.

Tovell left the room, saying he had an idea, and returned after ten minutes with, surprisingly, the Provost, Sir Ivor Evans. 'Nothing will be allowed to delay our building programme,' said he, and launched into the attack, reminding us of the meeting at Mullard Ltd to finalise the purchase of Holmbury, when he had been assured that "the house could be moved into tomorrow". 'Now you are asking for £30,000 to get it ready!' But with that said he got down to business. First he listened to a plan to make money by selling off the headmaster's house, but fortunately he dismissed it. He turned to Tovell's 'idea', which was to borrow £12,000 from a College fund and use that to get started on the essentials, the rewiring of the house, which was at the top of the list, requiring most of that sum. An extra maintenance man would be hired. Massey said that the group would go down and do as much as it could, painting walls and so on. Of course it wasn't paint the house needed but conversion. However the mood of the meeting lightened, and optimism crept in. It was an optimism severely tempered on our side by the knowledge that the group was immersed in a huge programme of equipment development, for the OSOs, OGO-E, ESRO 1, OAO-C and a clutch of rocket payloads. It was a terribly tight programme as it was. What would happen to it now?

Ten days later Massey, Boyd, Willmore, Davies and I stood with Tattersall and the Bursar in the empty skeleton of Holmbury House, despondently viewing the scene. The Bursar, who, surprisingly, was making his first visit, held in his hand a UGC letter containing another bombshell. 'Should a university spend its own money in place of a UGC building grant it would thereby forfeit its right to furniture and equipment grants.' Since the total of the two lists that I had drawn up and costed far exceeded the largest building estimates, scientific equipment alone coming to £132,000, it would clearly be out of the question to touch Tovell's £12,000. But there was a rider in the UGC letter "Where a project already underway would be held up for lack of

plumbing, electricity, etc., the UGC might make a special exemption from the ban.” So that was to be our lifeline. We toured the building, hoping to convince the Secretary that we should recruit not one but two or even three maintenance men, but he wasn’t having that. And, in any case, where should we find them? We had already been advertising an existing vacancy for three months without success.

Immediately another difficulty arose. Willmore, now installed at Holmbury, wanted others there too, and he proposed moving down those needing only offices - and, of course, heat, and light, and power! ‘Impossible,’ said the Bursar; ‘UGC contracts are let on a “vacant premises” basis, no inhabitants, no work in progress.’ We were now at the nadir of our fortunes. Boyd, Willmore and I called on Massey at his home to ask his backing for a stay of execution at Gower Street and a change in the Bursar’s stance, but he wisely saw that he could not act. We discussed the final solution, to drop Holmbury altogether; Willmore wouldn’t have been sorry because his heart had never been in the choice of so remote a spot, but we all realised in our hearts that matters had gone too far to back out now.

From that moment onwards, from mid-September 1965, the clouds began to lift. Soon the news filtered through that the Government restrictions, mediated by the UGC, were by no means as Draconian as feared. By early October we were exempted from the ban and the contracts were free to be let just as soon as they could be negotiated. What is more, I found the architect quite ready to arrange with whatever contractor was selected for a few residents to move in and migrate around the building as the conversion proceeded.

WATER, WATER, EVERYWHERE

Already we had one arrival, TADIC. This was a piece of digitising electronics, built for the group by EMI at no cost, for use on rocket analogue telemetry tapes. John Raitt’s data handling team had been forced to give up space in College and had brought the kit down to Holmbury. I had selected the north-west bathroom, later to become the electrical drawing office’s dark room, as the room least likely to be disturbed by the builders. Coggins and the Spanish gardener got to work, stripping out the bathroom equipment, taking out the pipework, making good and painting the walls, putting down linoleum on the floor. It looked very presentable, it had given Coggins something constructive to do, and it made the first contribution to the plight of the embattled group at Gower Street. Power for the digitiser was supplied down a cable from our single power point on the landing. With this first step taken we began to prepare to move Willmore’s supporters down.

One aspect of the TADIC room worried the young man who came to work in it. ‘How do I know,’ he asked with remarkable prescience, ‘that those water pipes that Coggins took out have been properly stopped off. I don’t care for the thought of TADIC being flooded.’ A good point, and I put it to Coggins. He was beside himself with rage. What right had these boys to question his workmanship? I tactfully withdrew the implication. In early October, with optimism in the air, I decided that it was time to bring the water supply back on and perhaps even the heating. With the help of Ted Pullen and Ken our electrician handyman, we cautiously released the ball cocks one by one in the three water tanks under the roof. We posted ourselves around the building in case of trouble. Suddenly, a shout of alarm from Mercedes in the kitchen. She could hear water flooding down the wall in one of the main rooms (GO7). She hurried to mop up. As she knelt in one corner the ceiling above her came down in a torrent of water, fortunately without injury to her. A second downpour began in the corridor outside. It was all coming from under the TADIC equipment. When we lifted the floorboards we found a 2-inch water main open at a joint where its companion length had been unscrewed and removed. It was exactly the oversight that the young man had feared. Another less alarming leak occurred from an

imperfectly sealed pipe in the bathroom later to become my office. The water ran down from the ceiling below, but no plaster fell. Mercifully the beautiful ceiling in the library was not below a bathroom.

By the end of October 1965 sufficient new electrical wiring was installed in the main building, with power becoming available section by section, to allow bedrooms to be furnished for temporary use by four research students. On 1 November 1965 I moved to Holmbury together with Molly, our new secretary. We set up an office in the library with Willmore and, on his occasional visits, Boyd. Mrs Coggins cooked the first meal. A month later, as the electrician continued his work, a heavy storm over a week end flooded the boiler hole once again. Our handyman Ken took one look at the mess on the Monday morning and walked out, leaving Jim Coggins reluctantly to cope.

Our first secretary Molly was soon followed by Libby and later Jane, both of whom remain part-time with us still (January 1991). Mrs Andrews came as our accounts clerk, and remained until her sad death a few years ago.

THE FIRST NOEL

Ten days before Christmas the Willmores organised a party. A bar was set up in the hall, and the common room, like the rest of the building an unheated, dirty and draughty shell, was transformed. Two days later the Coggins put on a Xmas dinner in the common room. There were guests, too, from IBM. We were negotiating the purchase of a terminal to link us to an IBM 360 at the future UCL computing centre. We had that very morning learned from our visitors that what was first to have been only an electronic link doing no more than reproduce punched cards, could now, if we could afford it, be an intelligent terminal, an IBM 1130 with a stand-alone computing capacity added to the card transmission. With our UGC equipment list now complete, vetted by the College and submitted, we were in urgent need of an increase to cover the 1130. The UGC were quite amenable, given the support of our assessors, one of whom was Peter Fowler at Bristol. 'Of course I will agree,' said he, 'it's lost in the noise anyway.' His co-assessor, the Director of the Rutherford High Energy Laboratory, agreed too, and we got approval for our IBM 1130. So, following that first Christmas dinner, we turned an important corner in the business of data handling.

BUILDING WORKS BEGIN

There was a long pause in the conversion through January and February. As so often happens, the uncertainty about finance had taken the steam out of the preparations in the Bursar's office, and now, with money available, they were caught unprepared and had still to go out to tender. There was no central heating and those of us working at Holmbury made do with such electric fires as we could bring from College, but it was on occasions bitterly cold. On almost the last day of February the builder, Arthurs of Dorking, moved in, and work began. Doors were wide ajar, dust and dirt was everywhere, but the weather soon turned balmy.

Gethyn and Adrian Timothy, two research students from Gower Street, had moved down and set up a laboratory in the south-east corner on the first floor. They organised a party in March 1966 in the library, that being the only habitable place. They staged a competition for a laboratory coat of arms. I took along my creation, an Achievement, since adopted by the Social Club. The coronet above the shield is the Ariel 1 satellite. The supporters are, on the left, HMG, a lion rampant, on the right, UCL, looking not entirely sure that it likes what it sees. The shield is quartered. As UCL had no emblem, being a college of the University of London with no charter in its own right at that time, I used the latter's emblem. The sun and stars depicted some of our

research concerns. The Welsh harp was the Guinness emblem. The daffodils represented Holmbury, as spring had by then sprung. Above it all, and emphasizing our growing indebtedness to NASA, the American eagle brought rockets in one claw, data tapes in the other.

Davies had made a far-sighted planning application to the local authority a year before. In it he gave an indication of future developments, including a new laboratory block and student accommodation blocks sited in the kitchen garden. It seemed an appropriate moment to set out in some detail the plans for a laboratory block, though one to be built on the Gladstone Walk - Gladstone having been a frequent visitor to Holmbury House in the previous century. The building was so sited that it could be linked to the main building. The College had a considerable building programme in hand at Gower Street, and it obligingly put our block on its long list of major works. The architects were authorised to draw up and cost a plan. The block was to incorporate a large mechanical workshop on the ground floor. The architect even obtained outline planning permission from the local authority, to lapse after ten years, and they constructed and presented to us a model. That was as far as things went. Shortly afterwards the UGC changed its arrangements for funding buildings, and the College found itself without a penny to support a single new major project anywhere.

THE FIRST FIRE ALARM

Drama erupted one day in early 1966 when a plumber at work on the water supplies under the roof set the roof lining alight with his blowtorch. The building foreman, a large, powerful man, plunged into the smoke-filled loft, where he successfully extinguished the blaze, perhaps preventing major damage. The part-time Cranleigh fire brigade had had to gather its men before it could set off, so they were some time in arriving. They found a prostrate foreman who, one moment jovial and relieved, had the next collapsed from the stress and the smoke. It was now the turn of the ambulance service to arrive and take him away for treatment.

WATER AGAIN

Water was our constant preoccupation, one way or another, during the first years at Holmbury. Shortly after moving in to the headmaster's house, Gerry Willmore was doing the household washing when the water supply failed - the storage cistern in the loft had emptied. Enquiries of her neighbours elicited the information that, because of our remoteness and height, the water supply was constantly failing. At least that was the story passed on to me, and I took it seriously. After all, we should be bringing down to the laboratory in a few months' time six or more water-cooled vacuum-pumping systems. Constant water failures would be intolerable. There seemed nothing for it but to become self-sufficient. I hit upon the idea of tapping water from the ornamental pond below the Gladstone Walk to feed a tank of 100 gallon or so sunk in the ground outside the G.05/G.07 laboratories. So Coggins was enlisted with as many others as we could find to lay two large-diameter pipes underground to link pond and tank. One pipe fed the tank, bringing its level to that in the pond. A row of electric pumps immersed in the tank circulated the water to the various vacuum systems. The return flow was discharged into the second pipe at a level well above pond level, so that the return flow was established. The arrangement worked well, provided that sufficient algacide was added to the pond water to kill the summer algae that otherwise clogged the vacuum systems. Then, little by little, cooling water began to be taken from the main water supply without anyone ever encountering problems. As far as I was able to establish later, our efforts had been quite unnecessary; the water supply to Holmbury rarely if ever failed!

The architect had paid particular attention to the surface-water drainage arrangements around the main building, finding them antiquated and in need of replacement. Indeed the underground piping and drainage was everywhere rotting away. The property still has a number of underground storage reservoirs, though whether these were interconnected at any time was not clear. Certainly leaking pipes made themselves known from time to time with a subsidence here, an audible coursing of underground water there. To absorb the surface water, newly piped from around the house, a sump, made of large pre-cast concrete rings, was to be formed in the sunken garden. Months earlier a burst mains water pipe in the forecourt had revealed, under the attention of Coggins and the gardener, a slaty ground structure which the architect took to be ideal for drainage. As the huge concrete rings were being lowered from their transporter the drainage contractor, who was at work preparing the site, came running over. He was digging into clay, an impossible medium for a sump. The concrete rings were put back on the trailer and taken away. We realised then that the forecourt had been formed sometime in the past as a platform on the sloping hillside using material brought in from elsewhere. The surface water had, instead, to be piped away under the drive and lost through a series of sunken leaky pipes radiating out across the field.

Years later, when the access road was being constructed behind the garage block, it was discovered that a grassy bank that formerly sloped down to the roof of the old boiler room was artificial also. The bank concealed a large steel cylinder that had been laid on its side immediately behind the rear wall of the boiler house, perhaps to hold fuel oil. The unsuspected presence of this cylinder, and of a large aperture between it and the wall, had completely ruined an earlier attempt by a forward-looking College surveyor to seal the boiler room against the constant incursion of water. He had arranged to have liquid concrete pumped from inside through the back wall so as to form an impermeable membrane over the concealed rear exterior. This operation, which lasted for weeks, had, all unknown to the contractors, merely created a large mass of concrete under the tank. When the first autumn storms struck, the flow of water into the boiler room became torrential, and water began to find its way for the first time into the cellars of the main house. It is an ill wind that blows no good. A major effort followed to settle the problem. The access road was cut through, a drainage 'curtain' was formed well below the new ground level to divert water, either underground or surface, that was heading down hill towards the house. The black hole under the boiler house was abandoned and replaced by the triple-boiler installation in the garage block. It was, of course, something that should in an ideal world have been done ten years earlier.

THE FINAL MOVES FROM GOWER STREET

By May 1966 I had planned, in consultation with the Assistant Bursar, the architect and the builders, a phased conversion programme to permit a step-by-step move of the group from Gower Street to Holmbury over the summer, with priority centred on the main building. The mechanical workshop completion was to be left until August, an arrangement that seemed to meet everyone's needs. It was a shock therefore when Willmore, now de facto head of the group in view of his residence at Holmbury, demanded that the workshop be moved earlier. I refused to go back on my agreement with the Bursar's office, and from then on my relations with Willmore were permanently soured. When, towards the end of August, the workshop building was ready, it transpired that after all there need be no hurry, and the move was not made until the middle of September.

By the end of August 1966 the builder was moving out and the electrician and plumber had little more to do. The research students had cooperated well with the builders, moving from bedroom to bedroom as the work progressed. Now they were moving into the flats in the garage block. The domestic flats were being occupied. The drawing office and electronics workshop staff had

moved down, the general office was functioning, and the kitchen, pristine in its stainless steel and white paint, came into use after the Bank Holiday. Thus the Mullard Space Science Laboratory came into being, nominally at least, on 1 September 1966.

The staff making the move from Gower Street were given a good measure of financial assistance by the College. Few could find affordable housing near by. Horsham proved the most attractive source of good, reasonably-priced housing, with Cranleigh gaining popularity as its housing supply increased. Some staff travelled daily by train to Dorking and were picked up by Ted Pullen in a van fitted with banks of wooden-slatted seats. As the months went by the number making use of this shuttle service decreased and when it fell to one only the service ceased. Recruitment of technical staff had been in progress for months at Gower Street, mostly of people living reasonably locally, and it continued, so numbers grew throughout 1966.

THE OPENING

The Mullard Space Science Laboratory was opened formally on 3 May 1967 by Dr F.E. Jones FRS, Managing Director of Mullard Ltd. The laboratory was en fête, though not so the weather, for it snowed. Fortunately a marquee had been erected on the terrace lawn, and there the platform party and guests assembled for the ceremony. Sir Harrie Massey took the chair, Robert Boyd spoke. Then Frank Jones, F.E. as he was universally known, cut a ribbon across the entrance to the common room.

Following the opening, some 50 guests and a similar number of the press were taken on guided tours of the laboratory. There were two satellites on display, the Ariel 1 replica, on loan from the Science Museum, and an OSO prototype, flown to this country and set up in Room G07 courtesy of NASA and the Ball Brothers Research Corporation. None of us had foreseen that the OSO would not go through the front door. As there was, in those days, no access road to the rear, a mobile crane was called in. The spacecraft was hoisted over the wall and trundled around the front of the house to G07, where doors installed by a far-sighted architect were wide enough.

Elsewhere there were actual Skua and Petrel rockets, courtesy of Bristol Aerojet Ltd, a Skylark telemetry and tracking bay, courtesy of RAE Farnborough, and items of recovered rocket hardware, picturesquely ruddy-brown from their impact with the Australian soil. In the laboratories we had demonstrations of space instruments and equipment, and it is of interest to list them, for they bring out the projects then at the forefront of our 1967 laboratory research. The absence of flight instrumentation was as noticeable then as it always has been and will be, for if an item of flight equipment is sufficiently completely assembled to be viewable it can rarely be spared for an exhibition.

In the ionospheric laboratory (GO 7) an electron temperature probe was being checked on the bench, and the short path-length ion mass spectrometer (Chapter 6) was shown being calibrated in a vacuum system. In the astrophysics laboratory (GO 5) a parabolic X-ray reflector was under test in the huge vacuum chamber known as 'the white elephant', (Chapter 5), an iron-55 source, proportional counter and a pulse-height analyser were set up to show how an X-ray spectrum is obtained, and the test rig for the stored-charge image reader (Chapter 6) was shown in operation. Upstairs in their laboratory the Timothys demonstrated the calibration, in a 2-m vacuum grazing-incidence spectrometer, of their scanning ultraviolet spectrograph, to be flown on the stabilised Skylarks 501 and 502. In the environmental laboratory the ill-fated shutter mechanism for the Copernicus instrument (Chapter 7) was shown on test in the new ultra-high-vacuum system, watched over by John Holmes. A test was demonstrated on the vibrator, a moving-in present from Hawker Siddeley, negotiated by Tom Patrick. The drawing offices, the first floor

computer room, and the electronics and mechanical workshops were included in the tours. The guides for the guests were Doug Davies, Keith Norman, Lyn Henderson, Uri Samir, Peter Sheather and Bruce Woodgate. For the Press they were Peter Willmore, John Raitt, Tom Patrick, Farouk Ragab, John Herring, Arthur Newton and Robert Boyd.

Mullard Ltd footed the bill for the entertainment of guests, both at Holmbury and in the evening at a formal reception and dinner in the Senior Common Room at College. There the Provost, Lord Annan, was in the chair, to introduce the speakers. Mr M.O. Robins spoke first, deputising for Professor Sheppard who, as chairman of the SRC's Space Policy and Grants Committee, was expected to confer official blessing on the MSSL, which would thereafter be ever more dependent on the good will of the SRC and the SPGC, and their successors. Speeches by Sir Harrie and F.E. Jones followed. Mullard Ltd had put their public relations office at our disposal and thanks to them we stepped our way unerringly amongst the thorns of protocol, foreseeing most potential disasters, and, very importantly, giving the fullest possible attention to the Press, making sure they were well wined and dined.

The MSSL was now launched, with a large programme of instrumentation construction well under way. Besides the many rocket payloads which formed the group's staple diet for so long there were advanced instruments under development for the NASA Orbiting Solar Observatories, for the fifth of the Orbiting Geophysical Observatories and for the third Orbiting Astronomical Observatory.

WORKSHOP ORGANISATION

The MSSL steadily built up from this time onwards a reputation for engineering excellence, based on its two teams, headed by engineers with the highest professional standards. It was always Boyd's aim that this should be so - he was, like myself, trained initially as an engineer, moving over to physics later. He therefore had a particular regard for the engineering side, first at College, later at Holmbury. This emphasis on making things was greatly disliked and criticized by certain theoreticians in the Department at Gower Street.

There was a certain difference of opinion at Holmbury in its early days about the rôle of technical support. It was not that there was opposition to it. The question was whether technical support should be concentrated, or decentralised; with the latter arrangement, individual scientists or groups of scientists would have their own dedicated technical support answerable only to them. Large laboratories usually end up having both, central workshops and individual support, often with the emergence of small one-man workshops kitted out with small machine tools. Boyd's view, and I supported it because I had seen so much of the problem elsewhere, was that, in a group the size of MSSL in the late 1960s with perhaps 45 scientists and technicians, it was better to concentrate resources. A case was made by the proponents of the decentralised system, but I think it is true to say that Boyd, having heard and read the case, ignored it. Harry Goddard and his brother Len remained in the main laboratory to provide a small element of dedicated support though, and the time came when they too set up a small workshop in the garage block for Len, with its small machine tools. That existed for some years until it was realised that we were probably breaking the Health and Safety Regulations by allowing Len to work on his own with rotating machinery. So it had to go.

COMPUTING ARRANGEMENTS

When the IBM 360 data centre at College opened for business and our 1130 was linked to it, we were in need of an entirely new service, the transportation of cards and printout. At first we sent

our material by British Rail Red Star service to Waterloo where it was picked up by a minicab driver and taken to College, the Centre there having a 24-hr service. Output came back in the small hours in a similar way. We continued, too, to use the Imperial College computing centre via the same minicab. But the British Rail service was very badly run and soon our loyal minicab driver was making the journey to Holmbury and back each night. All this was necessary because the data link transmitted punched-card data only. All computer input in those days was by card; the VDU had yet to arrive.

An end to the minicab driver's nightly journeys came when we were given by the SRC a more powerful data link into the Rutherford Laboratory's IBM 360 installation, from which we received our output, printing it overnight on a line printer.

When OGO 5 had been launched in March 1968 a large flow of magnetic tapes reached Holmbury to add to the numbers already acquired from Ariel 1 and OSO 4. More arrived with the OSO 5 and OSO 6 launches. The individual groups endeavoured to cope as best they could with the huge problem of interpreting the tapes before they could even begin to wrestle with the scientific information carried on them, and it became ever more clear that the laboratory needed to re-create its scientific computing group which had faded away in the years after the move to Holmbury. With the College beginning to look for staff savings and the SRC unwilling to allow us to increase staff numbers, it was difficult to move rapidly. But the necessary changes in staff recruitment and disposition were slowly made as opportunities arose.

When the IBM 1130, in its first-floor room, came to be replaced with the much more powerful data link to the Rutherford Laboratory, the first major shift round of accommodation took place. Hitherto the electronics workshop had been located in the elegant ground floor room in the south-east corner, but that room was now chosen as the new computer room. The move out by the electronics group to a number of laboratories on the ground and first floors was made possible by the significant change in the design and construction practices. The point-to-point wiring technique which had employed so many wiremen for many years was fast disappearing. In its place came printed-circuit boards, microchips, and a much greater emphasis on the drawing office, where the circuit designs were transformed into lay-outs which in turn became the circuit boards. An electronics drawing office appeared in place of the 1130, and the number of wiremen fell.

The new ground-floor computer room now became a fresh focus of activity. The punched card was still the only means of input, and the pressure on the sole punched-card machine grew. A second was installed in the cupboard under the stairs, then a third in a small room on the first floor. The corridors came to be lined with batteries of card cabinets.

When the Copernicus launch was imminent the OAO management at Goddard made a DDP computer available to us as a data link and that, too, was installed in the computer room, together with the NASA TWX electronics rack. A service counter appeared in the computer room, behind which Alan Hames and his staff tended the Rutherford link and its fast line printer. So much printing built up that resident students were paid to supervise an early-morning print run. Output was stacked in orderly rows upon the counter, and throughout each working day there was a constant coming and going. Visits by Rutherford staff kept us in touch with developments in the computing field, particularly about that much-longed-for but as yet not-available technique of interactive computing. The first screen appeared, on which we could develop graphical displays. When ready the graphs could be printed, but only at the Rutherford Laboratory, the output reaching us by post many days later.

As a member of a computing advisory panel to the Director of the Rutherford Laboratory, I had seen the pressures on the SERC's central computing facility grow month by month as more

outside groups, linked to it by data lines as we were, fought for more and more computing time. The staff of the computing centre wanted nothing better than to provide for these demands by an ever bigger installation. The customers, however, were starting to ask about the possibility of being provided with local computing power coupled with access to the mainframe. Some found their own funds to install local machines, which then came on line to the centre. Gradually the notion began to take hold at the centre that, wasteful in resources though such a development appeared to them, it would be tolerable provided it was carried out under their careful supervision. A plan was hatched to advise the SERC that a distributed system would be possible, even desirable, if supervised by them, and that it should be based on a number of GEC 2050 computers. This recommendation had the virtue that it stayed in line with the Government's recent ruling that computer purchases using Government funds must be from UK manufacturers. Indeed a Government agency had been set up in Norwich through which all proposals for computer purchases would be channelled in order to ensure obedience to the ruling.

When I brought back the news to Holmbury, that we might in due course be blessed with an SRC-funded GEC 2050 computer the reaction of the computer group was outrage. The machine had only a fraction of the power of an equally-priced DEC machine, and would be a dead-end investment, whereas the DEC range was already expanding. I am very glad to say that they got their way. They made their case to an SERC review committee, and, though their objective of meeting not only present needs but all foreseeable needs was not supported, they got their interactive DEC PDP 11/70 system. What is more, there were no objections from the Norwich office. Far from it, for, as that office told us, if all the orders for minicomputers were forced to go to GEC there could be waits of years, for the planned production in no way matched the potential demand.

With the interactive system there came in through the front door VDUs in increasing numbers, and out through the back door went punches, cards and card cabinets; no tears were shed at their departure. The DEC machine had its own demands - a fully air-conditioned room and a user area. So another change came about. The SRC grant for the machine included air-conditioning equipment. College funds were found to enable power supplies, partitions and the like to be provided. In due course our first colour-graphics facility appeared in the user area, funded from the Ariel 5 project. It was, I suppose, a system about which the least said the better. Yet it was bought only after the most careful research by John Ives, and it seemed at the time that his choice was the right one.

The PDP 11/70 soon proved too small to meet the growing demands at Holmbury, and this despite the fact that we remained on line to the Rutherford computing centre. Once again application was made to the SERC for funds, in January 1983, and once again they met us half way. It had been an unfortunate fact that the DEC 11/70 was within months if not weeks of being superseded by the VAX when our order was placed. The VAX had the huge advantage of being a 32-bit machine against the 11/70's 16-bit architecture. It was the VAX that had been chosen shortly afterwards for the Starlink network, which we could not now join because of the bit incompatibility.

Our new application was for a VAX 11/785. The SERC gave us an 11/780, delivered in March 1985. We were at once able to connect to the JANET network. At the same time our use of the Rutherford facility dropped away sharply as the RAL changed to a new operating system. We were from that time on effectively independent, an inevitable consequence of the change to interactive working using so-called minicomputers of hugely increased power and ever diminishing size.

A restless Chris Rapley in 1980, deeply involved in SMM, had a vision. 'We need a distributed computing capacity - many small machines around the building with their own local capacity, but linked to a mainframe.' A marvellous conception, but so remote, for who would fund it? We were to see it all come.

WE GET A CLEAN ROOM

Another important change came about in 1981 with the addition to the laboratory of its first new building, the clean room. There had been a pressing need ever since the group moved to Holmbury for a clean area, but there simply was no space available within the existing four walls. As we undertook ever more ambitious instrumentation developments our collaborators from the USA and Europe grew increasingly critical of our inadequate integration facilities. So in 1979-80 we approached the SERC for funds. The answer was guardedly optimistic, but there could be no question of them providing a building - only the University Grants Committee could provide buildings to universities. But the almost complete absence of UGC building funds meant that there was no prospect whatsoever of our getting a clean room that way. The answer was to apply to the SERC for funds for a clean-room facility, a piece of equipment - and not a very expensive piece of equipment as scientific equipment goes these days. There appeared to be a "window of opportunity" due to under-spending elsewhere in the Council's activities from which the necessary special grant might be forthcoming. Plans for a clean room were drawn up in collaboration with the College Bursar, valuable discussions were held with the RAL's own surveyor, specifications for air quality and for temperature and humidity control were agreed. It was interesting to see that what we were designing a kind of ultra-low-speed wind tunnel. Air was to be circulated continuously through a filter the whole width and height of an end wall, carried down the length of the clean room, picking up particulate matter on its way, then carried upwards into large ducts in the roof area and so back to the filter. Some fresh air was introduced at that point and the temperature and humidity adjusted also. This is not an altogether simple operation as was graphically portrayed by the temperature and humidity recorders when the system was first run up - there were huge involuntary excursions of both until the necessary corrections were made to the control system. I recall with some embarrassment my sceptical reaction to a request by Bill Gilford that a vacuum-cleaning system be installed with a single industrial unit working into pipework installed behind the wall panels and available at sockets around the laboratory. I was sceptical for two reasons. One because I assumed that there would be no dust. In fact users bring in dust all the time and only that remaining airborne is removed; dust or dirt or grit reaching surfaces sticks and has to be removed by old-fashioned dusting! The other reason was simply the novelty of the idea - I had never heard of piped vacuum-cleaning. A few years later I was holidaying in a house in Vancouver with plumbed-in vacuum cleaning! One cannot imagine a simpler and more satisfactory arrangement, the suction being switched on by the very act of plugging the hose into a socket. A lesson that has to be learned over and over again is that an idea can be all too easily rejected on the superficial grounds of impracticability when the real objection is to its novelty! Another embarrassment arose over the specification of the dark room. At that time our thoughts were very much concerned with the 1-m diameter X-ray telescope then being planned and the very sensitive detectors to be tested inside the darkroom. These were quantum detectors and therefore when light was excluded the blackout had to be so good that not a single stray quantum of light found its way to the detectors. Every possible source of light leakage was thought of and dealt with. Then the College surveyor spoke up. 'What about the fluorescent lights? Whenever I go down to my kitchen in the middle of the night the fluorescent lights are glowing brightly. Surely they will produce plenty of your quanta, or whatever you call them.' We hastily changed our lighting specification.

THE DIFFICULTY OF FORECASTING THE FUTURE

The College authorities had seen our stay at Holmbury as likely to be relatively brief. Boyd knew that he could not obtain from the SRC any undertaking of continued support. It was always clear that our future depended on our own successes, on the willingness of future Councils to support space research, and on Government policy. Who could say how that policy would look in ten years time? Indeed ten years had seemed in 1966 a reasonable, even optimistic, estimate of our likely tenure of Holmbury House. The programme of NASA satellite experiments upon which we were engaged would be over in ten years and, in 1966, there were no bankable prospects of continued work. Least predictable of all was the later turn-round in the SRC's attitude to other funding. The SRC at that time, and for many years after, was adamant that, having agreed to finance any scientific programme on the basis of dual support, it would only countenance support by a third body on the understanding that the amount received should be declared to them and deducted from their grant. In 1966, therefore, the College authorities made their assessment of a reasonable level of investment in Holmbury against a guess at a likely stay of ten years. We were in many ways generously dealt with, particularly in view of the unfortunate financial crisis that blew up in mid 1965. When, ten years later, the MSSSL was not only still alive but flourishing, the College was able to re-assess our situation differently, even though by that time the Government had begun its assault on University funding.

TWO SPECIAL VISITORS

Some while after our move to Holmbury, a visitor looked in - Guinness's niece, who had spent the war years at Holmbury. She toured the laboratory and was delighted to see the use we were making of the building. 'Could my uncle have known how this house would one day be used,' she said, 'he would have been thrilled.' She wasn't so pleased to see in one laboratory (GO5) the 'Adam' fireplace, as we knew it, a magnificent marble piece then in use by Harry Goddard as an electronics rack. 'I bought that just before the war from a house in Portland Square,' she said, 'you must rescue it and sell it.' Eventually the Bursar found a buyer, a dealer from near Sion Park, and he confirmed its provenance

Mr Mullard himself once put his head around the door to say hullo. He had started his company by making large radio transmitter valves for the Admiralty during the World War I. He fell foul of the patent laws and a court action in the 1920s robbed him of any part in the company's subsequent activities. So his connection with the MSSSL was a nominal one indeed.

5

INSTRUMENTS ON NASA SATELLITES

EXPLORERS 20 and 31

The ion mass spectrometer on Ariel 1, for which Boyd and Willmore had devised an improved ac mode of operation, gave many advantages over the usual dc method of identifying the dominant ion species hydrogen, helium and oxygen. Following the success of the instrument on Ariel 1, they were invited by Bourdeau at GSFC to provide an identical instrument for the top-side ionospheric sounder Explorer 20. His interpretation of the sounder measurements depended upon a knowledge of the local ionospheric parameters, and the UCL probe would help him in this regard. A spherical mass spectrometer was provided, to be mounted on a stem in a similar position to that on Ariel 1. The satellite was launched in August 1964. The same type of instrument, together with an electron analyser, was provided, under Gordon Wrenn's management, for NASA's Explorer 31, launched in November 1965.

ORBITING SOLAR OBSERVATORIES

One of the great successes of NASA's early scientific programme was its series of Orbiting Solar Observatories (OSO) based on a spinning satellite with an electrically-driven bearing holding a 'sail' which pointed solar cells and an instrument box constantly at the Sun through the illuminated portion of every orbit. Not only was the box pointed but it could be scanned in a raster pattern across the solar disc. Other instruments in the body of the satellite, known as the 'wheel', glimpsed the Sun once per 2-second revolution. For these and all of its subsequent scientific satellites NASA opened its Announcements of Flight Opportunities to the international community at large, and by 1965, that is, by the time that the move of the group to Holmbury had been decided, NASA had accepted MSSL proposals for instruments to be carried on three OSO spacecraft.

OSO 4 was launched in October 1967 carrying two MSSL instruments in the wheel, one measuring the total flux of soft X-rays from the whole Sun, the other measuring the total flux of 304Å (He II) radiation. OSO 5, launched in January 1969, carried a scanning X-ray instrument in the pointed box for a detailed examination of the solar disc; X-ray reflection optics were used for the very first time in orbit. OSO 6, launched in August 1969, carried, again in the wheel, a grazing-incidence polychromator measuring resonance-line intensities in the extreme ultraviolet.

The X-ray spectroheliograph on OSO 5 deserves special mention for those reflection optics. Towards the end of the 1950s, Boyd and Willmore had, in common with others in the USA and this country, felt it inevitable that sources of X-rays outside our solar system existed, but the question was whether they would be strong enough to be detected. It was clear that if the Sun were to be placed at the distance of the nearest star it would not be detectable in X-rays, certainly not with the techniques then in use.

In the USA, Friedman at the Naval Research Laboratory and Giacconi at Harvard, both with active rocket programmes and both involved in X-ray detection, had the same conviction. They found much support amongst the US astronomical community, leading to discussions about the most profitable sources to study. Inevitably the Crab Nebula was high on the list. Both had difficulty initially in obtaining funds to support speculative research, but by 1961 they were planning suitable rocket flights. Giacconi's historic discovery, almost certainly of Sco X-1, was

made in 1962, with an instrument nominally designed to look for X-ray fluorescence from the Moon. An active debate was taking place at that time about the nature of the lunar surface, with the possibility of a manned landing in mind, and Giacconi was contributing to it, whilst not unaware that his detector might, serendipitously, encounter something a good deal more exciting.

Back in 1960, however, more than two years before Giacconi's discovery, Boyd and Willmore whilst working on the satellite UK1, started to give thought to the possibility of looking for X-rays from stars. Their plan was to use a set of nested parabolic reflectors concentrating X-rays on miniature proportional counters. The Copernicus instrument (Chapter 7) grew out of this concept, as did, a little later, the OSO 5 X-ray spectroheliograph. The delays to the OAO programme were considerable, whereas the smaller OSO satellites went ahead with remarkable speed, and it was the OSO 5 reflector which was the first to be fabricated and flown. It is worth recalling that the Ball Brothers Corporation of Boulder, Colorado, who designed and built the OSOs, had come into existence after developing and building the Aerobee sun-pointing system. The firm's founders were then working at the University of Colorado. Buoyed by their success they left to set up in business on their own, eventually becoming the Ball Brothers Research Corporation, later Ball Aerospace.

William Glencross was given the task in 1965/6 of producing the OSO 5 and OAO-C (Copernicus) reflectors. Sanford handled the detectors, then being made at 20th Century Electronics at New Addington, and he became increasingly involved in the reflector manufacture. The method chosen was to form each long parabolic reflector by the electro-deposition of nickel on a parabolic mandrill, so replicating its surface. The mandrill, which had finally to be knocked out of the nickel tube, was to be given the best possible surface so as to minimise the subsequent finishing of the reflector. Rank Taylor Hobson of Leicester, known to Sanford from his time at the University there, were to finish the mandrill, that is they were to grind the parabolic surface to the required dimensions and with the required surface finish. They used their own specialist surface-measuring instrument, the Tallysurf. The nickel deposition on the finished mandrill, and the final polishing of the reflecting surface, following the rather traumatic operation of mandrill removal, was undertaken by AWRE Aldermaston. It was inevitable that many delays and difficulties should be encountered in blazing such a trail, and there was neither the time nor money, particularly with OSO 5, to guarantee the quality of finish which the reflectors required. Indeed there was no X-ray facility in the UK in which to test the performance of a reflector at grazing incidence, neither during the final polishing at AWRE nor after integration with the detection system. Only when we came to the OAO-C reflectors were we to discover, as we shall see in Chapter 7, how poor the OSO 5 reflector was likely to have been.

It is true that we had a test facility of a sort, in room GO 5 at Holmbury, not entirely affectionately known as the 'white elephant'. An X-ray source on a rotating mount was operated at one end of a large-diameter glass vacuum chamber, perhaps 2-m long, encased in protective plastic windows. Harry Goddard had used the vacuum chamber at Gower Street, and, having brought it to Holmbury, had become its custodian. He also built the rotating X-ray source. A reflector and detector were supported at the other end of the vacuum chamber and by rotating the source in a circular path an annular source was simulated. The divergence of the X-ray beam was too great, however, for such tests to give any reliable measure of scattering; indeed I was never sure what the value of the white elephant was, if any, and when it was eventually dismantled and turned to another purpose there was no requiem in its memory.

The actual performance of the OSO 5 reflector in flight could be estimated from its response to isolated points of X-ray activity on the solar surface, but this had to be something of a circular

argument, for there was no independent means of knowing the shape and size of any X-ray source. From later Copernicus experience it seemed likely that the angular response of the reflector to a point source flared out in the wings, much reducing the expected angular resolution.

The integration of the OSO 5 instrument into the pointed box on the OSO 'sail' required a long stay in the USA for John Herring, who worked for his PhD on the project, and shorter spells for Sanford. The box carried two instruments side by side, one from us, the other from NRL, and it was at NRL that integration of the two instruments with the box took place. In orbit, the box was scanned in a raster across the face of the Sun to give contours of X-ray intensity. The detectors were proportional counters and so the contour maps could be presented in several energy bands. The instrument, which had been designed and developed jointly by MSSL and Leicester University, operated well and provided data from which, for the first time, daily plots of solar X-ray activity were published until January 1973 by the World Data Center at Boulder, Colorado, in their Solar Geophysical Data Bulletins. NASA terminated data reception in December 1973, but the community of solar physicists later found the data so useful in support of other solar projects that they persuaded NASA to re-activate the spacecraft in mid-1974. For one more year, therefore, we resumed the preparation of the daily spectroheliographs for publication in the Bulletin. This task was undertaken by our growing data-handling group, in particular by Alan Hames. It was a novel experience to have an MSSL instrument in orbit in a passive satellite, all in good working condition but not in use, then to have it brought into use again on command from the ground.

The three OSO projects were, for us and all experimental groups involved, not only ambitious but also very necessary for the healthy growth of solar physics. Yet the instruments were limited in what could be achieved. They were limited physically in terms of weight, size and power, and technologically in terms of their detectors and electronics systems. The various US and UK groups that later built the instruments for NASA's Solar Maximum Mission owed everything to what had been learned with the OSO satellites. The spare MSSL OSO instruments have been handed on to the Science Museum, a testimony to these early scientific endeavours. The data have been discarded. In terms of accuracy, spatial resolution, time resolution and spectral resolution they have long ceased to be of value.

ORBITING GEOPHYSICAL OBSERVATORY

An early scientific achievement of the GSFC's satellite programmes, particularly that using the Interplanetary Monitoring Probes, was the discovery and initial charting of the Earth's magnetosphere. To exploit this work, NASA used the large geophysical observatories, designed and integrated at GSFC to carry instruments measuring particle densities and magnetic fields, some in low polar orbit (POGO), others in elliptical orbits ranging out to 20 Earth radii (OGO). MSSL's electron temperature and density instrument, in a project managed by Keith Norman, was re-designed for the exploration of regions of low electron density far out in the magnetosphere, and was carried on OGO 5, launched on 4 March 1968. OGO 5 operated successfully for several years. The probe yielded large quantities of data, all reaching MSSL, as with satellite projects generally, on magnetic tape. From these data it was found that the plasma density in the magnetosphere falls at the so-called plasmopause to very low levels indeed, a conclusion being reached at about that time by Carpenter in the USA from whistler measurements. The low electron densities measured from OGO 5 often proved indistinguishable from the local "atmosphere" of electrons emitted by the spacecraft itself under solar ultraviolet irradiation. Nevertheless OGO 5 was an invaluable early lesson in working on a major project and in dealing with the growing complexity of the data-handling task, and it paved our way to the European Space Agency's GEOS missions.

An unusual feature of the OGO 5 mission was the launch from the Western Test Range in California, where Keith Norman and his supporting team spent some weeks throughout the final checks. On two occasions when modifications to the Pye electronics box became necessary, Keith drove to Los Angeles airport and put the electronics box on an overnight flight to London. Next morning Ted Pullen, then our driver, cleared the package through Customs and drove with it to Cambridge. The repair done, the reverse procedure had the electronics box back into Keith's hands with the barest delay. Indeed Keith found that the turn-round was quicker than for a Californian group whose laboratory was only on the other side of Los Angeles!

6

SCIR & SPLMS

STORED CHARGE IMAGE READER

Whilst still at Gower Street, Boyd and Boksenberg had asked themselves how ultraviolet spectra taken aboard a satellite such as the Orbiting Astronomical Observatory might better be recorded and transmitted to ground. The system adopted by Spitzer at Princeton for his large instrument to be carried aboard OAO-C in 1972 was extremely slow, the spectral lines being exposed into one of a group of photomultipliers, one line at a time. Boyd and Boksenberg set to work on an invention, a stored-charge image reader, an electrical analogue of a photographic plate in as much as the ultraviolet spectrum would be exposed simultaneously over its full length, and the resulting 'exposure' read out electrically. The 'plate' was to be a thin, wide strip of insulator on a metal backing, curved to the Rowland circle of the spectrometer, with a sensitive layer deposited on top. During the exposure the UV photons would be sufficiently energetic to release electrons. The electrons would accumulate where they were released, so forming an electrical 'image' of the spectrum.

Now came an interesting idea. The charge pattern would be read out by the use of a vibrating metal knife edge which would be scanned across the sensitive surface. The knife edge and the metal backing would form a rapidly-varying capacitor. Electrical charge on the surface would result in an oscillating output voltage which would be detected and telemetered. The sensitivity of the device would be very high compared with a photographic plate, which, in any case, could not be used other than with a physical recovery system. I was familiar with the capacitive read-out idea because it had been employed by a colleague at Bristol to measure contact potential differences.

Boyd and I visited the National Research and Development Centre, the Government-funded office with the job of encouraging the patenting and exploitation of inventions, to ask for their assistance in patenting the invention and for help and funds to get industrial involvement in its further development. The outcome of this approach was that a patent officer was set to work to see whether the device could be patented - it later transpired that it could not - and collaboration was arranged with AWRE and with Hilger and Watts Ltd who manufactured a vacuum ultraviolet spectrometer. We already possessed one such spectrometer which was in use by the Timothy partnership. NRDC succeeded in convincing Hilger and Watts that it would be in their interest to donate a spectrometer for the purpose of the development and to modify it to house the SCIR instrument. AWRE were to build the very precise scanning mechanism. MSSL was to develop the detecting surface, the vibrating knife-edge system and the associated electronics, the mechanical side of the work being taken on by Harry Goddard. The collaboration was managed by Arthur Newton.

Nothing went well. Goddard found difficulty in producing a surface that was sufficiently smooth to be scanned at the closest possible spacing, the output signal amplitude being dependent on the reciprocal of the spacing. He had also to make a laboratory rig that would enable him to test his prototype device in advance of the AWRE mechanism becoming available. A servo system to maintain the spacing was tried, but this now raised the complexity well above the resources available. AWRE never produced a satisfactory scanning mechanism, though in retrospect that may not seem surprising in view of the problems that Goddard was encountering. Hilger and Watts misunderstood what was being asked of them, and although we eventually received a

vacuum spectrograph it was not sufficiently well modified to meet our needs. Once again I suspect that they were not getting the stimulus they needed from us because we had started the collaboration too soon and without sufficient manpower.

Boksenberg, who had not moved from Gower Street with the group but instead made occasional visits to Holmbury, was not directly involved in the work. However he began to ask himself why the device should be confined to the ultraviolet. Would it not be possible to use a photo-sensitive surface that would behave similarly at optical wavelengths? With one electron released per incident photon the device would be ten or a hundred times as sensitive as a photographic plate, and its use at the focal plane of an optical telescope would turn the Isaac Newton telescope at Herstmonceaux, for example, into the most powerful astronomical telescope. He put the idea to the then Astronomer Royal and Director, Sir Richard Woolley, who was sufficiently enthusiastic to find money and staff to support a separate development programme at Holmbury. He went as far as to tell the Press that he had a secret weapon which would put the RGO in the front rank of astronomy! Boksenberg began to appear at Holmbury more frequently, and work went ahead with much enthusiasm. Then there came disappointment. The expected capacitive signal was not appearing. The problem proved to be a fundamental one, as was eventually pointed out to Boksenberg by visitors from an industrial firm interested in the photocopying process. He had assumed that ionisation was taking place in the photo-sensitive surface, but he learned that it was only excitation. The electrons were not free but remained bound to their parent atoms. Boksenberg and his group spent a few last months endeavouring to ionise the excited atoms by applying an intense electric field across the measurement gap. To prevent sparking they covered the photo-sensitive surface with a film of insulating oil but without success. The work was closed down on both projects, without anything to show for so much expenditure of money and effort.

Boksenberg remained at Gower Street thereafter, but he continued to think about this brush with the attractive topic of a detector which would respond to individual photons. One day he sat in on a talk by Professor McGee of Imperial College about his high-gain image intensifier. McGee had worked for EMI Ltd in the 1930s on the development of the modern television camera. Now at Imperial College, he was developing his image intensifier, an advanced form of photomultiplier with several stages of gain and with electron optics, making it an imaging device. In 1962 McGee had invited one of the BNCSR Working Groups to see the device, his aim being to win space research funds for further development. The sight of individual photon events being recorded on the output screen was then quite remarkable. Later it had been my task to visit McGee with the Committee's regrets that it could not divert space research funds to support him. It was, I recall, a closely argued decision, and could so easily have gone the other way.

Now, some five years later, Boksenberg found himself listening to McGee's account of the hurdles to be overcome before he could incorporate within the vacuum envelope of his intensifier tube a read-out device. Boksenberg saw, as McGee spoke, that there was no need to wait. It should be possible to use a television camera to record the image on the output screen of the intensifier, the splash of a very large number of photons corresponding to the arrival of one photon at the faceplate, and then to employ computer processing to find the centroid of the event. The result a few years later was his remarkably successful Image Photon Counting System. The device used a McGee intensifier working into a Plumbicon television camera, a descendant of the EMI camera tube to which McGee had contributed so much.

How interesting that it was Boksenberg who was to bring the two devices together in this way, not McGee, the originator of them both. Boksenberg took some pleasure in pointing out what he had done, using existing pieces of equipment to create something entirely new. His contribution was the optical coupler, the centroiding system and, of course, the imaginative leap. But he was

pointing out a lesson which had already been demonstrated in several aspects of the UK space programme, both on the rocketry side and the instrumental side, but which still continues to be learned the hard way. It is that when stepping into a novel field of research one should resist if at all possible the temptation to invent an equally novel way of taking that step. Use existing techniques with which one is familiar. Don't push the technology further than is absolutely necessary. Be conservative - it always pays off in the end.

SHORT PATHLENGTH MASS SPECTROMETRY

In the early 1960s ionospheric research was a major element in most rocket programmes, with photochemistry being a topic of particular interest to Massey and Boyd. In the course of the US research an argument arose about the possible existence of quite large cluster molecules in the E-layer of the ionosphere, mostly hydrated ions. If these clusters did exist they would not in general be detected, the argument went, because the clusters would break up in the shock waves produced by the large mass spectrometers then in use on rockets. At UCL, Boyd had a research student, Alan Rogers, working with him on a laboratory mass-spectrometry technique which, they hoped, might one day be used in the lower ionosphere without the creation of a significant shockwave. They had devised a radio-frequency mass spectrometer of very short pathlength, using a gridded sensing head built as a thin disc. By giving the frame of the disc a tapered leading edge and flying it edge on, the sensor should create only a small disturbance. The very short pathlength ensured a negligible risk of a collision during the measurement.

With the Rogers-Boyd instrument still only a laboratory device but possibly a suitable subject for a patent application, Boyd found himself attending a scientific meeting in the USA. Amongst the participants was R.S. Narcisi of the US Air Force Cambridge Research Laboratories, an ex-Air Force officer turned physicist. He was not only what might fairly be termed a tough cookie but also a most successful experimentalist. It was he who had made recent claims about having detected cluster ions using a cooled quadrupole mass spectrometer. Now, still not accepted as a member of the US community of ionospheric physicists, he found himself under attack for his presumption. Worse, he was under attack from a woman! He was therefore delighted to find Boyd on his side, if not defending him at least attacking the attacker. This led to an exchange of views with Narcisi at which Boyd put forward the RF mass spectrometer as a likely means of validating Narcisi's claims - that is, if funds could be found for its development into something suitable for flight on a rocket. Narcisi was very much a man to put his money where his mouth was, and he offered an AFCRC contract and a number of rocket flights. Boyd accepted gladly.

The MSSL was thus launched on one of its first projects to be funded from an outside source other than the SRC. A research student was recruited, L.J.C. Woolliscroft. I became the MSSL contracting officer and, for a while, Woolliscroft's PhD supervisor in succession to Boyd. Work began in May 1967. The 10-cm diameter sensor grids, of which there were to be three in front of a collector disc, were electro-formed by Cathodeon Ltd. The electronics were designed and built by Pyes of Cambridge; the rf range was 20 to 40 MHz. Woolliscroft busied himself building a laboratory test rig, with an ion source based on a Kunsman filament. The first experimental sensor revealed the instrument's Achilles heel - the third grid, spaced only a quarter of a millimetre from the collector plate and biased at 100 volts, made with the collector an efficient capacitor microphone! A fourth, screening grid became necessary, apparently removing the microphony signal, but at some loss of ion throughput and therefore of measurement sensitivity.

Thereafter, construction of the flight instruments went ahead. We had hoped to fly them in pole position under a jettisonable nose cone but as Narcisi later took that position our sensor went on one of two booms. The rockets, two of them, were Nike-Iroquois, launched from Eglin Air Force Base, Florida, in May 1971. For the first flight we asked for and were given, by a now

sceptical Narcisi, a launch at D-region sunset. This choice was based on two considerations. We knew that a daylight launch would result in the release of photo-electrons from the second grid. With an accelerating voltage of 100 volts outwards on the first grid, spurious ions would be created in front of the instrument, which it would then proceed to detect. After dark, on the other hand, the instrumental sensitivity would be insufficient to detect the low night-time ionisation densities.

In the first flight, microphony swamped all signals during motor burning but thereafter disappeared. Ion signals were very low and only the highest-gain output was usable; this meant that the only signals available to us were very noisy. In the second flight, which was brought forward into daylight to raise the signal level despite the risk of photo-ionisation, there was a sensor failure during motor burning and all was lost. Our tentative identification of ions detected in the first flight was, in terms of their mass numbers, 55 (Fe⁺), 36 (K⁺), 28 (NO⁺), 21 (Na⁺ or Mg⁺), 18 (water) and 16 (O⁺).

It was, all considering, not a wholly disastrous outcome, but the fact was that the signal/noise ratio was so low that we should have had no confidence in our identifications, other perhaps than of NO⁺ and Fe⁺, had it not been for Narcisi's own identifications in earlier flights. We had suffered badly from low sensitivity, down to one fifteenth of that expected, because we could not get grids of the expected transparency, and, of course, we had had to increase their number to four. Nor was there an opportunity to cross-check our results with the quadrupole mass spectrometer as Narcisi's interest had moved on from positive to negative ions.

Narcisi had done extremely well in his research in the years during which we were developing the rf instrument. His identifications of positive ions, not only of clusters but perhaps more importantly of metallic ions, had quickly become accepted, and he had long ceased to have any need of confirmation of his early results. Norman took over after Woolliscroft's departure with the hope that more might be made of the instrument, and he flew two more models in Petrel rockets at South Uist, but eventually the project was dropped. Perhaps we had taken to heart the words of John Slater, the successful business tycoon of the 1960s who had made himself a millionaire by buying up failing businesses, throwing out the loss-making parts and selling the profitable remains. 'The trouble with most people,' he had said, 'is that they clutch their failures to their bosoms.'

COPERNICUS

Boyd and Willmore's proposal to NASA of an X-ray 'telescope' using, for the very first time, reflecting optics was accepted in 1963 and assigned to the third Orbiting Astronomical Observatory, OAO C, later named Copernicus. The principal package was the Princeton ultraviolet telescope, a very large instrument for those days. The MSSL package was accepted for a 'piggy-back' ride, the term no doubt underlining the fact that it would be allowed to prejudice neither Princeton's scientific objectives nor the launch schedule. In the event the OAO team at GSFC, where the spacecraft was assembled and tested, gave us every possible help and consideration.

Our package was composed of three parabolic reflectors of different sizes. The critical grazing angle for reflection is a function of X-ray wavelength and so by suitable choice of geometry each reflector was given its own working range, 3-9 Å, 6-18 Å and 20-70 Å. The two shorter-wave channels were supplied with pulse-height analysers to give a measure of the spectral content, the third channel giving only a total energy measurement. In a fourth barrel an optical star sensor provided a means of obtaining an angular measurement to some suitably-placed bright star to aid the determination of X-ray source position. Finally an additional X-ray counter was placed behind a simple collimated slit to provide a measurement channel at 1-3Å that was independent of the reflectors. This was to prove to be a move of quite remarkable foresight. The electronics system was again designed and made for us by Pyes of Cambridge.

By choosing reflectors, Boyd and Willmore had not only provided collecting apertures of quite useful size for those early days but had made it possible to employ very small X-ray detectors. By keeping detector volume so small, the troublesome background signals caused by particle radiation in orbit would be minimised. To get some measure of the problem of background radiation it is worth noting that even with such small detector volumes the background radiation flux would have risen to damaging levels when the OAO spacecraft entered the South Atlantic anomaly. It was therefore necessary to plan to switch off the MSSL detectors by ground command for the appropriate period twice each day. Elsewhere the background was still appreciable, making signal chopping necessary. A two-bladed chopper, driven by a stepping motor, was turned in programmed steps during periods of observation so that every spell of counting against a source plus background was accompanied by a similar spell of counting against the background only. This constant reference was made necessary by the ever-changing particle background as the spacecraft circled the Earth. The chopper was to play a very decisive role in determining how we should use Copernicus throughout its long life, for it jammed during the first year and blinded the two principal channels thereafter!

The design and construction of the mechanical structure of the MSSL instrument was carried out at UCL and later at MSSL under the design leadership of Tom Patrick and Peter Sheather. It was the largest project to have been undertaken, requiring a major effort at a time when all of the instruments for the three OSO spacecraft, for OGO 5 and for three ESRO spacecraft were under development and testing. The reflectors and detectors were again the responsibility of Glencross and Sanford. The project manager was, for a short period, Adrienne Timothy. She and her husband Gethyn had built and flown two ambitious stabilised-Skylark payloads, 501 and 502, bringing together solar ultraviolet and ionospheric measurements in a bid to solve all the outstanding problems in the chemistry of the ionosphere - or so Gethyn once optimistically put it. But despite two excellent flights, the scientific problems, though perhaps dented, had

certainly not been mastered. Now, with Gethyn starting two more equally ambitious rocket projects, Adrienne was weaned away from the partnership to participate in OAO-C. But the Timothys' eyes soon turned towards brighter prospects in the USA. Gethyn took a post at Harvard, Adrienne with AS&E in Boston and later at NASA HQ, Washington. Following their departure, Sanford took over the project management and remained with it throughout the spacecraft's operational life.

The construction and finishing of the OAO-C reflectors followed much the same path as with OSO 5, at least initially. The OAO programme had experienced a series of delays as a result of which many advantages accrued, with probably few disadvantages apart from the hugely inflated cost to NASA. One very significant advantage for us came from our access through the GSFC OAO management to a long-beam X-ray test facility at the Martin-Marietta plant at Boulder, Colorado. As the sophistication of X-ray optics advances in the future, X-ray autocollimators will no doubt become standard test equipment, if they do not exist in the USA already, but in the early 1970s no optical means existed for providing a parallel beam of X-rays for test purposes. Instead an X-ray source was operated at the far end of a long evacuated tube. The smaller the source, and the longer the tube, the better the approximation to a parallel beam. We were to construct a small facility of this sort years later to support the SMM project, after failing to convince the Appleton Laboratory that they should set up a central facility, but now, using the Martin-Marietta facility, Sanford was able for the first time to assess the quality of the MSSL reflectors in X-rays. The results were instructive and discouraging in equal measure. An initial discovery was that the test facility itself was comically faulty. The X-ray sources, of which there were several to give a choice of wavelengths, were being incorrectly operated. Instead of there being one small X-ray source on view at a time, a large area at the end of the tunnel was aglow with X-rays. Some prompt modifications soon put that right. The tests on our reflectors then showed how poor they were in terms of what might be called the point-spread function. There was far too much scattering. Back at Holmbury, Boyd headed a searching appraisal, with the conclusion that, as time just but only just permitted it, the reflectors should be manufactured again, from the very beginning. One is tempted to say from scratch, but that would be tasteless!

A new force in X-ray optics had recently appeared in the shape of Dr Franks and his group at the National Physical Laboratory, Teddington. NPL was the home of the ruled diffraction grating, and Franks was working on grazing-incidence techniques for use at X-ray wavelengths where surfaces needed to be polished to the same very demanding tolerances as we were seeking. He soon convinced us that he had the experience which we so badly lacked. He found that the final polishing operation at AWRE on the now-rejected reflectors had been on a lathe, with the reflector rotating and the polishing tool moving axially down the interior parabolic surface. This process was bound to throw up circumferential ridges on the reflecting surface, which, to X-rays at a grazing incidence of only a degree or so, would act as scatterers of, or even barriers to, the incident radiation. His technique, he explained, was much closer to that used in conventional optical workshops when shaping mirrors. For our job he would make up a conical array of sprung pads of pitch which would be stroked along the axis; there would be no rotary polishing action. He undertook to polish the new OAO-C reflectors in this way. Sanford therefore set the wheels in motion for the manufacture of a new set of reflector blanks. As each was delivered it went to NPL for the crucial polishing operation. Though the schedule was tight, the reflectors were completed in time.

Willmore returned from a visit to GSFC a year or more before the launch of OAO-C with some not-very-welcome news. The project was going to require of us an active role on the operational side. Hitherto we had had only to wait for data tapes to arrive from GSFC, the day-to-day commanding of a spacecraft requiring little experimenter input. OAO-C by comparison was to

be a large and complex operation with much to be done in advance by way of working out observing programmes. Such planning would include making decisions on spacecraft slewing operations to bring the telescope bore-sight with minimum delay on target by the use of the momentum wheels. It had been arranged that for 90 per cent of the spacecraft operational time the main telescope would be viewing Princeton's chosen ultraviolet sources. These were unlikely to be of much interest to us. For a few days a month, however, we were to be given full control of the spacecraft. It would be up to our staff to make all the necessary plans for the observing slots, to choose from the agreed list of targets the most convenient source to observe, to plan the required slews, bearing in mind the many restrictions as to what might and might not be done, and then to specify the necessary commands to be transmitted to OAO-C. At first Boyd and I tried to see how all this might be carried out from Holmbury but Willmore was adamant, and he proved to be quite right. The job required the closest possible liaison with the GSFC teams actually in control of the operations. We therefore set about acquiring the necessary financial resources, and identifying the staff who would spend weeks at a time at GSFC. Thus a new phase began, that of supporting satellite operations abroad, after launch. It has since become an accepted part of large projects. Fred Hawkins spent many months at GSFC. He learned the OAO command language and wrote software for use by our own teams as they prepared the observing command sequences.

Because of the substantial delays to the schedule, time had been available for GSFC to equip the OAO-C spacecraft with what later proved to be a highly effective inertial-reference system. For years the pointing of the spacecraft was, as with the two earlier OAOs, to have been by reference to a group of star sensors which were to be set up at specific angles for each observation. The spacecraft was then to be manoeuvred by use of its inertia wheels until the sensors had all acquired their optical targets. When the inertial system became available, GSFC and the OAO-C project manager, Joe Purcell, made the brave and costly decision to add it, keeping the star-tracker system as a back-up. The particular value of the inertial reference lay in the fact that each Princeton observation was to be an extremely lengthy affair, because of the slow sequential scanning of the ultraviolet spectrum, and so had to be carried on from orbit to orbit, with breaks as the source went into eclipse. Thanks to the inertial reference, the telescope would be able to pick up the source rapidly at the end of each eclipse, whereas the re-acquisition of reference stars on the old system would have been time-consuming.

Another huge benefit to MSSL from the delay was that NASA's small X-ray satellite Uhuru had from late-1970 been used to bring about a dramatic increase in the number of known X-ray sources, both galactic and extragalactic, as well as confirmation that many were variable. We were now in a totally different situation as regards our X-ray studies, for we had this wealth of Uhuru sources at our disposal. OAO-C would now provide us with a unique and powerful facility for their study, with high resolution in space and time. One is bound to wonder how many sources we should ever have seen without the aid of Uhuru, for the narrow field of our detectors would have made sky-scans a lengthy business.

Copernicus, as OAO-C was christened, was launched from Cape Kennedy on 21 August 1972, and the MSSL instrument was turned on a few days later. Such was the interest that the BBC sent their science editor and a camera crew to Holmbury. The main performer was Mike Cruise who had just had a successful rocket flight at Woomera! From launch the inertial reference proved a major success. With MSSL staff and students doing turn and turn about at GSFC the observational programme got underway. By and large the X-ray instrument performed well. There was disappointment that the long-wave channel was being swamped by an unexpectedly high level of background radiation. Comparisons with the background measurements of the other two channels showed that the source was ultraviolet light from the Earth's own corona - had this been foreseen the telescope could have been provided with a filter thin enough to

transmit the soft X-rays but sufficient to absorb the ultraviolet. Steps were at once put in hand to provide such a membrane over the soft X-ray counters on later projects.

Our work at Holmbury was very much assisted by the provision by the OAO management, who were thereby helping us above and beyond the normal bounds of their commitments, of a Honeywell DDP computer to which we interfaced a tape deck. The DDP computer was linked in to the NASA communications centre at GSFC through their London switching centre, who gave us both a voice and a data link through which were fed our Copernicus data. In one sense the link was superfluous - the airmailing of tapes was already well-established by earlier GSFC projects - but the close contact that it gave us with the day-to-day Copernicus operations was of great value, giving our involvement an immediacy which it could otherwise never have achieved. We were indebted also to Professor Spitzer who, seeing how successful our X-ray observations had become, doubled our allocation of time from ten to twenty per cent. We were also indebted to the Appleton Laboratory later for their invaluable support in stationing operations staff at GSFC. Still later the OAO management hired people locally. Sanford made frequent lengthy visits to GSFC and maintained excellent contacts with those in charge of operations, including Kupperian, the manager.

Copernicus was equipped with a comprehensive command system. To ensure that each assembly of commands reaching the spacecraft was correct in itself and would not hazard the payload or the spacecraft, it was prepared well in advance and submitted for checking by a special computer program. In June 1973, less than a year into the observations, our staff at GSFC reported a hesitant operation of the chopper and expressed their fear that it might become stuck in the closed position. Boyd decided after a careful review of the pros and cons that it would be best to park the shutter and forbid its further use. Sufficient data on the radiation background at all geographical positions and heights had been accumulated to make the science team feel fairly confident that they could thereafter estimate the background with sufficient accuracy. The instruction was at once relayed to the Copernicus operations staff - no further commanding of the chopper. Yet, inexplicably, a shutter command was sent quite soon afterwards and, as had been feared, the shutter jammed in the closed position, blinding the two shorter-wavelength channels. All attempts to dislodge the shutter failed. The spacecraft attitude was manoeuvred to raise the temperature of our instrument in the hope that that would somehow unstick what was stuck, but that did not help. With the soft X-ray channel already unusable we were reduced to the collimated single-slit detector which had so wisely been included. It was with this instrument alone that we continued our observations from June 1972 to the termination of Copernicus operations six years or so later.

To paraphrase Lady Bracknell, to lose one channel may be regarded as misfortune; to lose three looks like carelessness. But the loss of those three channels brought us an unexpected opportunity to concentrate our limited observing time on one particular topic - the long-term variability of X-ray sources. With the launch of Ariel 5 and its two MSSL X-ray instruments, two years after Copernicus, the value of our Copernicus instrument was enhanced for we were able to choose objects for study using both satellites. A joint observing programme with the US National Radio Astronomy Observatory was arranged and, when the International Ultraviolet Explorer, a US/ESA mission, got underway there was enough overlap to allow yet more coordinated observations to be made. Copernicus was even used in real time to observe a variable X-ray source which was to be examined using one of our high-resolution rocket instruments from Woomera. A telephone link from GSFC to Holmbury and a teleprinter link from Holmbury to the Woomera range head made it possible for the rocket launch to be 'held' until the Copernicus instrument showed that the source, which would go 'off' completely, had changed into an 'on' state.

Copernicus operations were terminated by NASA at the end of 1980 on the grounds of cost, after more than seven very successful years. Our instrument was still working well and profitably, but the cost to the UK of shouldering the data tracking and operations was too high for that possibility to be given more than the briefest attention. The analysis of the data went on for many more years. It was the arrival first of NASA's Einstein satellite and then, in 1983, of ESA's Exosat satellite, with their X-ray imaging telescopes, that pushed Copernicus data from centre stage.

But with Copernicus, Ariel 5 and then the two Exosat imaging instruments, we began to see from the early 1970s onwards a sea change in astronomy. The emphasis steadily moved from the establishment of new but largely independent branches of astronomy - X-ray, ultraviolet, infrared and radio - to an integration with optical astronomy, creating one hugely expanded discipline, astronomy in its widest sense.

8

ARIEL 5

The Ariel 5 satellite, built in the UK and launched by NASA without charge on a Scout rocket, was wholly devoted to the new and rapidly-growing field of X-ray astronomy. It was to become, in scientific terms, the most productive of the Ariel series. Amongst its six instruments, two built at Leicester University, one at Imperial College and one at NASA's Goddard Space Flight Center, were two MSSL instruments. One, Experiment A, built in collaboration with the University of Birmingham, surveyed the sky for X-ray sources in the 0.3-30 keV energy range. The other, Experiment C, measured the spectra of individual sources at 2-30 keV.

Because of the cooperative nature of the Ariel series of satellites, each proposed payload was put to NASA for its approval. Initially this approval was sought as much as anything to test the feasibility of what was being proposed. Later, from Ariel 3 onwards when the payload engineering became wholly a UK responsibility, it gave NASA a chance to check that the UK was not duplicating too closely its own scientific programme. When the UK 5 payload, which was to be devoted exclusively to X-ray astronomy, was proposed there was good reason to expect awkward questions. Giacconi (Harvard and American Science and Engineering, Boston) had not only been the first to detect extra-solar X-ray sources, he had built and operated the small but hugely successful Uhuru X-ray satellite. Now he was embarking on a new venture, the first imaging X-ray telescope for what was to become the Einstein satellite, and he and NASA (it never being clear to us which of the two was the more powerful) were expected to question the need to put yet more US money into launching and tracking a small British satellite for X-ray astronomy.

Willmore was appointed by the SRC to chair the scientific group, made up of Leicester, Imperial College and MSSL, which was to formulate a detailed scientific proposal, and he sensed from the outset that to succeed would require a proposal to NASA with some especially attractive ingredient. Whatever the actual chronology of his thoughts, he did produce what was required. The special ingredient, then very novel in a scientific context, was a proposal that the MSSL should provide a digital computer to service the complete payload. With this plum in the UK 5 pudding, the proposal survived a fairly tough inquisition. In addition a British offer of space on the satellite for a US instrument was warmly accepted.

Because of the advantage gained in reducing the energetic-particle background, to which the X-ray detectors responded quite as sensitively as they did to X-rays themselves, it was proposed that UK 5 should be launched into a low-inclination orbit from an Italian off-shore platform north of Mombasa (Kenya), so keeping the spacecraft below the radiation belts. In addition, the proposal nominated the Appleton Laboratory, then at Slough, to provide day-to-day operational control. Having won NASA acceptance, the project went ahead under close SRC supervision. The project management was undertaken by the Appleton Laboratory. The UK satellite contractor was Marconi Space and Defence Systems, Portsmouth, under contract to the Ministry of Defence who acted as agents for the SRC. The RAE became the R & D authority, carrying out technical monitoring and major environmental testing.

Whitehall's political thinking is based on the Roman philosophy 'divide and rule'. The idea of a UK version of NASA is an anathema to Government, whatever its persuasion, for it fears losing control. In 1964, so complex had the arrangements for Britain's space programme become, so scathing were the comments of the media when they sought information, that the Office of the Minister for Science found itself compelled to publish a pamphlet:

‘The organisation of space activities, covering quite separate fields which are the responsibilities of various government departments, is necessarily complex. This pamphlet explains the various responsibilities in the present organisation. Its aim is to help the press, TV, and others to route their enquiries directly to departments and organisations.....’

Of course the SERC is not Whitehall. Indeed by being moved to Swindon the SRC, as it was then, was carefully distanced from it. But the ethos remained, carried over by senior permanent staff drawn from Whitehall departments. Not only did these always very able men divide and rule instinctively, but they had to answer to former colleagues in Whitehall, who expected it as a matter of course. Many years later the Government’s decision to create the British National Space Centre suggested that at last the Rubicon had been crossed. But when the BNSC’s first director, Roy Gibson, resigned it became clear that nothing had changed. So the UK 5 project was blessed from the outset with a management structure of alarming complexity. Yet it suited we British so admirably that when, as we shall see in Chapter 12, Ariel 6 was planned the same management and control was adopted.

With two major UK 5 instruments to be designed, built and tested, the additional task at MSSL of producing a small, low-power general-purpose computer obviously was a challenge to the electronic engineers, but a challenge was never a challenge to Willmore if it did not include a substantial element of originality. He pored over computer manuals until he was sure he understood how a small computer, the PDP-11 for example, was organised. He next devised a scheme for a machine based on just three types of silicon chip, notional ones, that is, which would have to be designed and manufactured. Then he contacted Plessey Ltd who had just set up a new factory in Swindon where they were to design and produce the very latest in microcircuits. Willmore was impressed by their futuristic ideas. They foresaw a day when devices would have so much inbuilt redundancy that they could be made self-checking and self-correcting against internal faults. Plessey were impressed by Willmore’s grasp of computer technology and microcircuit design. After a series of visits to Holmbury by an accountant, a production manager and a single designer, who was clearly dazzled by Willmore’s brilliance - he told me as much when I met him socially years later - agreement was reached on the production and pricing of the three devices. They never materialised.

Ray Penney was put in charge of the computer design and production at MSSL, but his preparatory work was never put to the test. The impending disaster was forestalled by electronics staff at RAE who, in a timely move, proposed replacement of the computer with a much simpler logic device which they would tailor to the precise needs of the spacecraft. Willmore’s general-purpose computer might have led to something of lasting value or to a dead end. In all probability it would have been the latter, in view of the rapid development of microchip technology. As it was, a good management decision was made to drop the MSSL computer in favour of the RAE system which performed well.

‘Experiment C’ was managed by John Ives. The detector had a multi-cell geometry created from fine wires strung in an elegant frame and housed in a gas-filled chamber covered by a large domed beryllium window. It presented Peter Sheather, his team and the contractors, Twentieth Century Electronics with many constructional, vibrational and sealing problems, and it always seemed touch and go whether of the several models under construction there would be one entirely reliable for the flight spacecraft. In the end there was, and it performed extremely well, apart from one totally unforeseen, and probably unforeseeable flaw. At certain angles of attack, ionospheric plasma entered the satellite and caused breakdown of the high-voltage supply to the X-ray counter. As the problem came to be understood so it became possible to schedule the observing periods of the instrument so as to avoid the times of potential breakdown. A particular feature of the Experiment C instrument was its location on, but at a small angle to, the spin axis

of the satellite. The resulting modulation of the output gave positional information about a source with respect to the known spin direction.

‘Experiment A’ was managed by Arthur Newton, in collaboration with the Birmingham group. X-ray detection was by three large gas-filled cylindrical beryllium counters beneath a rotation collimator. The beryllium, which, like that on Experiment C, was machined for us at the Royal Ordnance Factory, Cardiff, where suitable facilities existed for handling that very toxic material, proved to be porous. Once again it seemed doubtful whether three cylinders could be found that would retain their charges of gas, but in the event they were found. The design, construction and evaluation of the large collimator grids that were located above the counters was a particularly challenging task. A Dutch firm undertook their manufacture by an electro-deposition technique. Before being mounted in their frames at Holmbury the grids had to be evaluated to ensure that the width and spacing of the bars, and therefore their pitch, was within tolerance. This evaluation was achieved by adapting a coordinate table, purchased earlier for another quite different purpose. Its movements in the two axes were driven by stepping motors, and these were now coupled to a small computer to obtain programmed movements. A microscope was mounted on the stage and as the grids were stepped on command beneath it an observer measured the deviation of each bar from the nominal position. It represented, I believe, one of the earliest applications, if not the earliest, at Holmbury, of a computerised measuring technique for use by the workshop staff.

For a few alarming days in the final stages of the functional tests on the rotation collimator it appeared to Newton that a disastrous shortcoming had been revealed in the spectral response of the detectors. An in-flight calibration system had been provided, using a small radioactive source which was swung on command into one side of the detector aperture. Newton found that when he tested the calibration system the detector output did not correspond to the known spectral characteristics of the source. After much anxious investigation together with colleagues, a very worried Arthur Newton reported to Boyd a serious counter malfunction which none of the group could explain and which threatened the whole success of the instrument. Boyd at once called a meeting of all those in the laboratory with experience of proportional counters. With characteristic determination and physical insight he went through the evidence, questioning everything, patiently eliciting from the detector experts one small fact after the other. It required a considerable degree of optimism, in the face of apparently overwhelming evidence to the contrary, that an explanation would be found, one that would exonerate the general principals behind the detector design. The explanation was found, and it was Boyd’s persistence and optimism that uncovered it. Each proportional counter had a single anode wire running almost its full length, held at each end by tensioning supports. The correct functioning of the counter required the release of a burst of ionisation by an incoming X-ray quantum in the undistorted radial electric field around the active length of anode wire. But the radioactive source had been illuminating the end of the counter where the electric field was distorted. Slowly the truth emerged - the calibration source was being wrongly positioned as it swung into place. A mechanical design change was all that was required. It was not a welcome matter so late in the project, but the difficulty had been eliminated.

Cruise had, as a research student, taken over from Willmore three Skylark payloads in which earlier versions of the Experiment A rotation-collimator instrument were flown at Woomera. Unlike Experiment C, a fully-functioning rotation collimator did not guarantee a successful measurement. Interpreting the data was peculiarly difficult and, at that time, very demanding in computer time, notably in dealing with the ‘aliasing’ problem. After launch the difficult Experiment A data analysis fell mainly to the Birmingham group, now headed by Willmore. He had left MSSL in September 1972 on being appointed head of the Department of Electron Physics on Sayer’s retirement. An attractive feature of the rotation collimator instrument was its

comparatively large field which meant that it complemented the multiwire counter. The former could find new sources, for closer examination by the latter.

The electronics for both instruments were designed and built for the laboratory by a group in the Pye Telecommunications Ltd laboratory at Cambridge. A very close working relationship had been built up with this group since Ariel 1 days, in particular with John Blades and Don Weighton. We had come to rely on their expertise not only to make what we wanted but also to tell us the best way to approach each new project, so that Pye became an extension of our own electronics design, development and manufacturing capability. The relationship ended when we and other space research groups who had come to rely upon them could no longer provide a sufficient flow of contracts to justify Pye keeping the group together.

The launch of Ariel 5 from the Italian San Marco platform by a NASA Scout 4-stage launcher on 15 October 1974 went well. It was that same platform from which the NASA Uhuru satellite had been launched, hence the Swahili word for 'freedom'. An optical star tracker on the rotation collimator, put there to aid X-ray source location, failed shortly after launch. Thereafter it was necessary when locating a new source to have a known X-ray source in the field.

The Appleton Laboratory control centre, working closely with the Goddard tracking system, provided an excellent operational service. On occasions staff from university groups including the MSSL assisted at the centre. The spacecraft had been provided with two means of attitude control, gas jets and a magneto-torquer, and by careful planning the gas was made to last three years or more whilst manoeuvres were carried out to meet the observing needs of the many users. When the gas was at last expended the magneto-torquer was employed alone. The spin axis of the spacecraft was thereafter limited to a thirty-degree band, but as this was a region prolific in sources the restriction was not serious. The attitude changes did, however, take much longer to bring about. In all this, the control centre staff were particularly skilful.

In March 1980 the spacecraft re-entered, after a period when, because of the difficulty in controlling the spin direction, useful measurements were all too few. Data analysis went on for at least two more years and at Holmbury it fell more and more under the general management of Jocelyn Bell-Burnell.

It would be impossible to summarise here the value of the Ariel 5 project to astronomy, but it was certainly very considerable. A particularly fortunate feature was that throughout the entire 5-year operating lifetime of Ariel 5, observations were also being made with the MSSL X-ray instrument on Copernicus, which could be brought to bear with great accuracy on targets of special interest to the Ariel 5 experimenters.

THE ESRO/ESA SPACECRAFT

The European Space Research Organisation's scientific programme was based initially (1962) on sounding rockets, but was soon extended to satellites, small and large. The first ESRO rocket flight of an MSSL instrument took place from Sardinia on a Skylark in March 1965. Thereafter, MSSL ionospheric, ultraviolet and X-ray instruments were carried on 47 ESRO rockets - British-built Skylark and Petrel rockets and French-built Centaure and Arcas rockets - fired from Andoya (Norway), Euboea (Greece), Kiruna (Sweden), Sardinia and Woomera.

ESRO 1 & ESRO 2

The first ESRO satellite to be planned, the polar ionospheric satellite ESRO 1, was overtaken during construction by the second, the solar X-ray and cosmic ray satellite ESRO 2 (IRIS), launched on 16 May 1968. It carried an instrument for the measurement of the total solar X-ray flux, prepared jointly by MSSL, Leicester University and Utrecht University, and was very similar to the X-ray instrument on OSO 4. ESRO 1A (Aurorae), launched on 3 October 1968, carried in its payload two MSSL electron and positive ion probes of the Ariel 1 type. The performance of the spacecraft was so satisfactory that the back-up craft was launched as ESRO1B (Boreas) on 1 October 1969.

ESRO 4

Spin stabilisation of small scientific satellites holds the spacecraft orientation constant over many orbits, but for plasma measurements it has the disadvantage that the orientation of a probe to the on-coming plasma is constantly changing throughout an orbit. Measurements made when a body-mounted probe is looking forward into the plasma stream are normal, but then, as the velocity vector veers round, they change until half an orbit later the probe is in the satellite's wake. Boom-mounted probes are less compromised, but even so they suffer peculiarly from an oscillatory voltage induced by the spinning motion in the Earth's magnetic field, as was experienced with the plane electron probe on one of the Ariel 1 booms. It was therefore of particular interest when ESRO planned two rather larger attitude-controlled satellites to be launched on Thor-Delta rockets. An instrument could thus be pointed forwards for long periods.

With one of these satellites, TD 2, in mind, MSSL set about the development of a large, high-sensitivity ion-mass spectrometer. Unfortunately the satellite development costs at ESRO grew well beyond what had been expected, and in 1968 ESRO abandoned the project, replacing it with yet another spin-stabilised small spacecraft ESRO 4, to be devoted to ionospheric, neutral atmosphere and particle physics. It was launched on 22 November 1972, with a payload made up of the large spherical, boom-mounted ion analyser by MSSL, a liquid-helium-cooled mass spectrometer by von Zahn at the University of Bonn, and particle detectors by Hultquist in Sweden.

John Raitt was responsible for the MSSL project, Peter Sheather for the large gridded, spherical ion probe, twice the diameter of its forerunners, eight times the volume and therefore a surprisingly big device, and John Blades of Pyes at Cambridge for the electronics. Incidentally, pictures of ESRO 4 show three booms and, apparently, three spherical probes. In fact there was only one working probe, the other two being dummies, placed there for dynamical reasons.

With an electrostatic probe, the energy of an arriving ion is largely determined by its mass and by the satellite's speed, which is well above the thermal speed of the ion. However the thermal energy is still sufficiently large to broaden the spectral line appreciably, and with the earlier body-mounted probes the ion peaks of hydrogen and helium were generally found to be indistinguishable. The original objective in going to the larger instrument size, before ESA changed the spacecraft design, was to increase the signal and therefore the energy resolution to the point where the hydrogen and helium peaks could be resolved. Not wishing to lose a satellite project, Raitt stayed with the change to ESRO 4, choosing the boom-mounted position, though uncomfortably aware that the increase in resolution brought about by the fourfold increase in collecting area might be cancelled out by the loss in resolution occasioned by the spin. In the event, that is what happened and so the hoped-for advance was never realised.

The high speed of a satellite through the Earth's magnetic field results in an appreciable induced voltage across the length of the boom. This voltage, which varies with the changing orientation relative to the Earth's magnetic field and falls to zero at the magnetic equator, is in general of a similar magnitude to the average value of the swept retarding or ramp potential. Furthermore, as the satellite spins the induced voltage reverses cyclically, and so appears as an alternating function superimposed on the relatively slowly-swept ramp voltage applied to the probe. With ESRO 4, the ion probe data had to be corrected for the effects of the induced oscillatory voltage during the data-reduction phase before the ion spectrum peaks could be extracted. It was this correction procedure which lost us the very advantage offered by the larger probe surface. For the sake of clarity I should add that the probe technique devised by Boyd and Willmore involved the use of one or two low-amplitude high-frequency (a few kilohertz) voltages which were used to measure the instantaneous slope of the probe curve throughout its comparatively leisurely large-amplitude sweep. These high-frequency voltages should not be confused with the relatively slow, cyclical ramp voltages.

The potential which an ionospheric or magnetospheric satellite adopts during its orbit around the Earth varies constantly with speed, ionisation density and temperature, and solar illumination, and its instantaneous value can very much effect the output of a swept-potential probe. The potential must therefore be taken into account during the data-analysis phase. To obtain a measure of this satellite/space potential, Raitt included with the ion probe an electron probe, a simple ungridded spherical probe with a swept retarding potential taking the probe well to each side of space potential whatever the satellite potential. When collecting electrons, which move very fast compared with ions, only a small collecting area is needed, and in this case the probe diameter was 1-cm compared with the 20-cm of the ion probe. The electron probe was situated close to the ion probe but not so close as to be influenced by its charge sheath. To be sure of this, Raitt added a command facility to switch the negative bias on the large probe to a larger value to see whether the resulting larger sheath would then envelop the small probe. This higher voltage resulted in no measured effect at all, and thereafter it was assumed that the small electron probe was never influenced by its big neighbour.

In the absence of an induced voltage due to spacecraft spin the current to an electron probe varies exponentially with the ramp voltage. The use of a logarithmic amplifier makes the variation linear and its slope then becomes a direct measure of electron temperature. The point on the ramp voltage at which the linear slope changes abruptly marks space potential. What could be simpler? In the presence of the induced cyclical voltage due to spin, the same correction during the data-reduction phase as with the ion probe is made and the effect of spin is removed. Today the procedure might be carried out in the spacecraft, but at some risk because the procedure has to accommodate the changing probe curves as the satellite varies in height, in angle of attack, in local time and in magnetic coordinates.

The small electron temperature probe performed its primary function of establishing space potential well, and also produced a wealth of other information. I joined Raitt in its analysis, and he eventually left me to it. The measurement of electron temperature once every 12 seconds, or about every 100 km along track, at first appeared very satisfactory, but later it proved so questionable as to throw up serious questions about our understanding of the behaviour of a simple retarding-potential Langmuir probe.

A feature of the discussions at the early technical panel meetings at UCL in the late 1950s on the rocket programme was the rivalry between Boyd and Beynon over their respective techniques for probing the ionosphere. Boyd's expertise lay in the use of Langmuir probes, Beynon's in vertical-incidence sounding, in other words in wave propagation. Boyd argued powerfully that the only sure way of establishing the ionospheric parameters was to go there with in situ probes. Beynon argued that probes disturbed the very thing one was trying to measure; better to use wave propagation which would tell all one wanted to know without causing any disturbing effects. One problem for we uninitiated was to see just how Beynon's wave propagation method could give the required information. Boyd's probes seemed easier to understand. Discussions about the two techniques often became heated.

Now, after 35 years, who seems to have been right? The very large amount of work with probes by the UCL group from the late 1950's culminated in 1972 in the ESRO 4 ionospheric experiment. Probe theory was thought by then to be well understood. Yet, though no fault in either the satellite or the probe electronics was ever found, the electron temperatures obtained were clearly in error, particularly at night when they were often twice too high. It later turned out that we were not alone in being puzzled. Brace's group at NASA's Goddard Space Flight Center had been experiencing much the same problem with their cylindrical retarding-potential analyser. This, incidentally, suggested that our problem did not lie with the choice of spherical geometry. In 1976 I read that Brace's latest probe, that on the Atmospheric Explorer AE-C satellite, had evidently worked correctly; the earlier problems had, it seemed, disappeared though Brace wasn't clear why.

The criterion for what constituted correct operation for a probe was a new method of probing the ionosphere, that using radar incoherent scatter, a wave propagation technique again of course, coming into use both in the USA and in this country, at the Royal Radar Establishment, Malvern. The RRE signal returns were used to give measures of ionisation density and electron temperature as a function of height up to the F layer, although, of course, at only the one site and only intermittently.

Early in the data-analysis phase on ESRO 4 Raitt and I reported in the literature that the first few comparisons between our satellite measurements and the radar measurements were encouraging. This optimism stemmed in part from there being so few reduced radar data coinciding in time and height with satellite passes over Malvern. Later, as more reduced radar data were made available, it became uncomfortably apparent that our worries about the anomalously high electron temperatures were all too real. I met Brace at Goddard to see whether he could yet suggest the reason for the improvement in the performance of his cylindrical probe, but he could do no more than re-iterate his published list of design changes. No one change could account for the improvement. Both group's probe techniques for extracting electron temperature involved measuring the energy distribution of the electrons. Hoegy, one of Brace's colleagues, suggested, perhaps ingenuously, that the faulty probes were in some way measuring only the high-energy tail of the distribution. If it were so, it were a grievous fault!

The Ariel 1, Explorer 20 and 31, and ESRO 1 probe measurements had undoubtedly been successful in terms of the new light that they were throwing on the behaviour of the ionosphere, and, as was becoming clear, the magnetosphere too, of which the ionosphere forms the very

lower boundary. Boyd and Willmore had been able to claim that their Ariel 1 measurements had contributed more to the knowledge of the upper ionosphere than all previous measurements put together. Yet here was a serious difficulty. The retarding-potential probe technique was shown to be at the very least more complex than its use by the group over almost ten years had led it to believe.

By contrast the wave-propagation method of measuring electron density from spacecraft went from strength to strength, though not in this country. Beynon's proposal in the mid-1950s for a radio-propagation experiment measuring the phase difference between a low-frequency signal which would be influenced by the ionosphere and a high-frequency signal which would not, had inspired Hazell at RAE by its elegance. Why not turn it into near perfection, he argued, by incorporating an RAE pulse telemetry system which was 'available', though not in regular use? The telemetry system was certainly beautifully suited to the precise timing of events, and that was what the Beynon experiment was about. So the pulse telemetry system was adopted, despite the fact that it wasn't supported operationally by WRE. Two large and powerful ground transmitters were contributed by the Admiralty, who, in a stunning show of altruism in 1956, offered to support the project with 'men, money and materials'. Sadly their enthusiasm quickly evaporated, their support was withdrawn, and soon they were even billing the project for the hire of the transmitters! In time the radio propagation experiment grew into the mother and father of all experiments. It had quickly been taken out of Beynon's hands because of its huge technical complexity and was thereafter carried out by RAE staff. When at last obtained, the results were gratifyingly good but they came about a decade too late to do more than confirm what was now well-established. By contrast the Atmospheric Physics group at the University of Michigan had developed a simple but effective radio propagation technique and were recommending, through COSPAR I believe, that all instrument packages on ionospheric rockets should use it for the routine measurement of electron density. By that time, however, interest at UCL in ionospheric physics from rockets had gone, in favour of magnetospheric, solar and X-ray physics.

One last comment before leaving the subject of ionospheric probes - over the years 1962 to 1974, when ESRO 4 re-entered, ionospheric data of very similar form were collected in almost unbroken sequence using UCL/MSSL probes.

GEOS 1 and GEOS 2

The Earth's magnetosphere was one of the notable discoveries of the space age. As more was learned it began to be apparent that complex interactions take place in the turbulent interior of the magnetosphere between energetic particles, mostly electrons and protons originating in the solar wind that have become trapped in the Earth's magnetic field, thermal electrons and ions from the ionosphere, and waves which propagate through the various regions of the magnetosphere in various modes. To unravel these complexities, the European Space Agency, the successor to ESRO from April 1975, devised the GEOS payload as a concerted investigation by many research groups, using a range of particle and magnetic-field sensors.

Though techniques had been developed by the early 1970s for measuring the high-energy trapped particles and, of course thermal particles as we had done with OGO-E, none was available for measuring an intermediate group of low energy or 'suprathermal' particles. Yet it was becoming to be realised, initially in the USA, that these would be an excellent source of information on wave-particle interactions in the magnetosphere. Wrenn had discussed the idea in the United States and on his return he took it up with his colleagues, in particular with Alan Johnstone. They began the development of a hemispherical low-energy plasma analyser, initially for use in the rocket programme. Then, when ESA requested proposals for its planned geostationary satellite GEOS, an improved version of the analyser was successfully proposed.

Gordon Wrenn managed the project. The instrument, a small box-like affair, needed to be well away from the other instruments and so was positioned at the end of a boom. It contained two analysers, one looking along the spacecraft spin axis, the other at right angles to it, the aim being to measure the fluxes of energetic protons and electrons in the range 0.5 eV to 500 eV, and to determine their pitch angles. The particles originate in the solar wind, leaking into the magnetosphere where they are trapped magnetically. They then spiral backwards and forwards along the field lines from one hemisphere to the other. As they approach a pole those with sufficiently large pitch angles are lost in atmospheric collisions. GEOS, sitting, as it was planned to, on a meridian above the equator, could be used to observe the comings and goings of these particles after injection, and their angular range.

GEOS 1 was launched in April 1977. It failed to reach geostationary orbit because of a rocket motor malfunction but valuable data were obtained from its unplanned highly-elliptical orbit. GEOS 2, its replacement, went into a very satisfactory orbit in July 1978 and performed well for several years. The data sets from both have been used to describe the physics of the magnetosphere - the sources of trapped particles, their convective motion whilst trapped in the electric and magnetic fields, and their eventual loss through wave/particle interactions.

EXOSAT

ESA's X-ray astronomy satellite Exosat carried a set of eight large-area, medium-energy X-ray detectors, one gas-scintillation medium-energy X-ray proportional counter and two low-energy imaging X-ray telescopes. It followed the outstandingly successful NASA Einstein satellite as the second astronomy mission to provide actual images in X-rays.

The original Exosat concept was developed out of an ESA-sponsored study held at MSSL at a time when the accurate location of X-ray sources, which are often not visible at optical wavelengths, was the astronomer's first priority. To that end, Exosat was conceived as a lunar-occultation observatory for which a highly-elliptical orbit was necessary. However, the great success in the USA with Uhuru and the emergence of practical techniques for the true imaging of sources using two-component reflection optics soon shifted the emphasis away from lunar occultation. ESA undertook a major payload re-design. One mission feature remained unchanged, however. The 4-day elliptical orbit proved a powerful advantage, for it permitted uninterrupted observations of eclipsing X-ray binaries over long periods in the presence of flaring and other intrinsic variability.

MSSL had originated the research and development of the low-energy position-sensitive detectors chosen by ESA for use with two low-energy imaging X-ray telescopes. There were two types of detector, one of each being available on command at the focal plane of each of two telescopes. ESA, having adopted the MSSL designs, insisted on assuming control of all further development, manufacture, testing and flight operations, with MSSL acting as consultant only. It was never a happy arrangement. It was arrogant of ESA to look down its corporate nose at an experienced experimental group and tell it that for instrument production its competence was not equal to that of ESA. It was foolish of them not to appreciate that, in so early a stage of the technology of X-ray imaging, production isn't production at all but a continuance of research and development - witness the experience with the Ariel 5 detectors where development problems were still being sorted out only months before the launch. It was wishful thinking on their part to suppose that in taking on the detector production they had in some mysterious way ensured that the prior development was complete. They compounded their error in a way peculiar to ESA, burdened as it was with its principle of *juste retour*, by breaking up the manufacturing task between three different commercial firms. A British firm made the detectors, an Italian firm built the associated electronic amplifiers. and a French firm provided contractual control. The MSSL team became science consultants, responsible for any shortcomings in the

design or performance of the imaging detectors but without being allowed any say in contractual matters. It was an exceedingly difficult time at MSSL, not least for Ian Mason who had managed the detector development and now watched over subsequent developments from the jump seat.

One example of the inadequacy of ESA's arrangements will suffice. At a point probably beyond three-quarters of the way to completion, Mason discovered that the Italian firm had misunderstood, or perhaps had never begun to understand, the function of the amplifiers which they had contracted to make, for they built them with too low a frequency response to match the detectors. Something had to be done, and quickly. But the Italians would not speak to him; as sub-contractors, they had been expressly forbidden to do so by the French. When Mason rang the French contractor he was rebuffed. It was too late to make changes, they said, and anyway they were working to a price, and changes would be out of the question; in other words the science was not their concern. Mason was eventually able to prevail upon the ESA project office to take some action, but it was, of course, the kind of thing that can ruin any scientific endeavour.

An acknowledged shortcoming of the X-ray detection techniques being employed in our low-energy counters, and indeed in all gas counters, was the low inherent spectral resolution. There had been a hope for some years that a solid-state detector would soon appear which would give the better resolution, but so far it had not appeared. An alternative technique with a promise of an improvement of two or three in resolution, enough to be worth pursuing, was the gas scintillation counter. The technique deliberately avoids creating avalanches which are at the heart of a proportional counter. Instead the electrons released in the counter gas by an incoming X-ray quantum are drifted to a region where they excite rather than ionise. The subsequent de-excitation is accompanied by the emission of light, that is, scintillation occurs, and it is this that is detected, using photomultipliers. The measurement of light output gives a better value for the number of electrons released, and therefore for the energy of the quantum, than does the measurement of the total avalanched charge. This is because of the statistical uncertainty of the gain per avalanche, of which there may be many in each event, again according to the quantum energy.

Work on a gas scintillation device had been going on for some time at MSSL when we became aware of a similar interest in the Space Science Division at ESTEC and at the Universities of Milan and Palermo. We therefore formed a consortium with them and proposed, successfully, an 'add-on' instrument for Exosat. In the course of time the work on this detector became concentrated at ESTEC.

Exosat was launched on 26 May 1983. The mission was generally a success. The medium-energy detectors developed at Leicester performed well and turned out to be the mainstay of the mission. The gas scintillation detector performed well, particularly gratifying for so new a device. But the low-energy detectors were plagued with high-voltage electrical breakdown problems. One detector only came up to scratch, the other three having to be nursed and had repeatedly to be taken out of use. To what extent were the faults attributable to our scientific designs or to ESA's inept contractual arrangements? I don't know. What is sure is that if ESA had contracted with the MSSL at the same level of funding as they did with their trio of firms, we would have been able to bring in industrial support on a substantial scale under our supervision, and we would have done a better job.

In keeping with an arrangement with ESA, MSSL was given priority access to the data, though this was arrived at only after a considerable battle, into which Boyd put all his considerable muscle. A new computer graphics facility was assembled on which to process the X-ray

pictures. The Exosat data thereafter became a uniquely valuable asset to the laboratory in its astrophysics research.

FIREWHEEL

The Max-Planck Institut at Garching initiated in 1977 a magnetospheric project called Firewheel, not a true ESA project of course, but making use of the opportunity of a ride on one of the Ariane test flights from the French equatorial range at Kourou. The intention was to release charges of barium and lithium at points between 9.5 and 7.5 Earth radii, and to observe the expansion of the plasma clouds and other associated phenomena using four sub-satellites. The Appleton Laboratory and the MSSL were invited in 1977/78 to provide one of the sub-satellites and to equip it with plasma diagnostic instruments. This proposition was particularly attractive to our mechanical engineering team who thereby undertook their first construction of a spacecraft, small though it was. The project ended disappointingly with the failure of the Ariane LO2 rocket, and a watery grave in the Gulf of Mexico.

METEOSAT

MSSL has an interest in the often damaging effects of charged particles in space on the operation of satellites. As part of an investigation into the source of operational anomalies that afflicted its Meteosat 1 meteorological spacecraft, the European Space Agency contracted with MSSL for Wrenn and Johnstone to provide an energetic-particle detector for Meteosat 2. The instrument was designed to detect particles in the energy range 50 eV-20 keV, at which energies surface-charging effects occur.

In the event no convincing correlation was found between anomalies on Meteosat 2 and the arrival at the spacecraft of particles in this particular energy range. It was therefore decided in collaboration with ESA that deep dielectric charging by higher-energy particles must be occurring, leading to the build up of comparatively high-voltage charges on dielectric surfaces, which might then discharge through neighbouring electrical components and cause damage. Wrenn having by now left UCL, Andrew Coates came into the project and under his leadership a suitable instrument was built up around a group of solid-state detectors provided by the US Los Alamos National Laboratory, to detect particles in the 40-300 keV range on the next Meteosat spacecraft.

Meteosat 3 was launched in June 1988. A strong correlation was then found with anomalies occurring on the Meteosat radiometer. This important confirmation of the early suspicions is now being reflected in new ESA projects. Particular attention is being paid to the need to screen components from particle bombardment. There was a bonus. With Meteosat 2 and Meteosat 3 in nearby geostationary orbits, simultaneous measurements of particle fluxes were made over the full energy range 50 eV to 300 keV, so providing a valuable data resource.

10

MORE ROCKET EXPERIMENTS

THE SPORADIC-E LAYER

The simple ionospheric probe devised by Willmore and his ionospheric group in the early days of the Skylark programme took a number of forms, spherical, cylindrical and planar. It was the latter, the so-called 'plate probe', that was often used because it could be let very simply and unobtrusively into the rocket skin. Though unable to provide an absolute measure of ionisation density, it was most useful in giving an ionisation profile, and in particular in detecting one of the oddest of phenomena, the sporadic-E layer.

'Sporadic-E' had long been known to radio amateurs as a transient phenomenon which provides anomalous reflection of radio signals from the E layer. One of the earliest Skylarks, SL09, fired at Woomera on the night of 19 June 1958, carried an instrument built by Sayers' group at Birmingham which quite fortuitously recorded an intense layer of ionisation at 100 km. The same layer was observed from the ground with an ionosonde. The structure of such layers subsequently became familiar to us, a very sharp boundary on the underside, a less steep fall-off on the upper side, the width no more than two kilometres. It was particularly fortunate that that Skylark firing took place during the International Geophysical Year, so that Massey was able to announce the observation at the Moscow meeting of the Special Committee for the IGY in July 1958. It was the only report at that meeting of an IGY rocket observation!

Clues to the origin of sporadic-E layers began to appear when Groves in this country and others in the USA began to publish measurements of wind speeds and directions at ionospheric heights. It became apparent that the wind direction between 90 and 150 km varies with height in a complex and generally irregular way, sometimes making a complete rotation, sometimes resulting in strong wind shears in direction and speed over quite a short vertical distance. These rather unexpected wind structures appear to be caused by the propagation of atmospheric waves, the so-called 'gravity waves' to distinguish them from other types of waves, over great distances. Not only is the rotational structure of the wind direction with height created by the atmospheric waves, but the phase propagates downwards with time, measured in hours. Over the years various attempts were made to link such wind structures with sporadic E, and to argue that in the presence of the Earth's magnetic field the motion of the ions as they are carried across the field result in $V \times M$ forces that would either lift or depress the ions, depending upon the relative orientations. A 'pinch' effect could result which, under the right conditions, would lead to a high concentration of ions over a comparatively small vertical distance. Furthermore the height of such a layer would steadily decrease as the phase of the wind structure moved downwards, exactly as is observed with ionosondes. Rees at UCL, working with members of the Flight Research Group at the Weapons Research Establishment, argued that a complete description of the electrodynamic forces on the ions required a measurement of both the magnetic field and the electric field. A rocket payload was therefore put together to make a comprehensive study of all relevant factors.

Skylark 922, fired at astro-twilight on 2 March 1971 at Woomera in the presence of a strong sporadic-E layer, carried a rubidium-vapour magnetometer, an electrostatic-field probe, four MSSL plate probes, two barium dispensers and a trimethyl-aluminium (TMA) dispenser. The TMA trail gave an excellent neutral wind hodograph between 90 and 170 km. The plate probes, for which I and Raymont were responsible, gave good measurements and located the sporadic-E layer very precisely on the up and down legs - finding the heights to differ by half a kilometre,

indicating a half-degree tilt of the layer to the horizontal. I had also developed a proposal by Willmore to detect the flow direction of the ions as distinct from the neutral atmosphere. The possibility of doing so arose because Skylark rockets were now spun up at launch and during the powered ascent. I found the neutral wind and the ion wind to be closely coupled up to about 120 km, and my measurement of the sharp change in wind direction at the sporadic-E layer was exactly the same as Rees's measurement from the TME trail - a very gratifying result in view of the novelty of the method I was using. Above 120 km the neutral wind and the ion wind became rapidly uncoupled, as might be expected. Good electric field measurements were obtained and Rees and the WRE group were able to argue that it is the electric field that determines whether or not a stable layer of ions can form, and at what height. The occurrence of such a layer, with an ion density many times greater than the background density, is only possible if ions other than oxygen ions are present, ones with a very much longer lifetime. The nature of these special ions had by then been established, principally by Narcisi (Chapter 6), as magnesium, silicon and iron, probably the remains of burned-up meteorites. The descending wind shears sweep up the metallic ions and bring them down as sporadic-E layers to a height below which atmospheric turbulence churns them away into oblivion. Sporadic-E layers seem therefore to be the product of Nature's vacuum cleaning!

STABILISED SKYLARKS

The Skylark rocket programme had been transformed in the mid-1960s by the introduction of an attitude-control system, which, after motor separation, pointed a rocket head at one or more chosen targets. There had been no lack of proposals from the astronomical and space community prior to that time for experiments requiring a stabilisation system. Butler at the Royal Observatory Edinburgh, Ring at Imperial College, Boyd at UCL had all made proposals that rested on there being one. In June 1960 discussions had been held between the BNCSR and RAE as to what might be possible. RAE had been studying the question but were unable to take the matter any further without the injection of money to promote a contract in industry. It was the point at which RAE was forced to say that if the BNCSR wanted new rocket facilities it would have to find some way of financing them. The lesson was not wasted, and in January 1961 after the necessary committee considerations the BNCSR put into its 1961/62 estimates an item of £10,000 per annum for three years to cover the development in industry of an attitude stabilisation system, under Ministry of Aviation supervision. With this assurance the group at RAE immediately recommenced discussions on the likely requirement and on a possible contractor.

At this point there entered a new factor, the UK Atomic Energy Authority, its Rutherford High Energy Laboratory at Harwell, and the Culham group of RHEL, the Controlled Thermonuclear Research Division. The Rutherford Laboratory was later to be carved out of UKAEA Harwell on the formation of the Science Research Council in 1965, taking with it not only the accelerators but also the Atlas Computing Laboratory. The subsequent merger with the Appleton Laboratory and the move of the latter's staff to Chilton created the RAL.

Amongst the staff at Culham was Robert Wilson, lately a member of the Royal Observatory Edinburgh, still at heart an astronomer. He led a team studying the physics of laboratory plasmas. He and his colleagues had earlier succeeded in obtaining from the UKAEA financial support for a series of ultraviolet studies of the Sun using rockets, on the grounds that what went on the Sun could well be very relevant to what might one day go on in a controlled thermal-fusion reaction. He and his director, R.S. Pease, went to RAE in mid-January to register their interest and to join, for the first time, the discussions going on there. In March 1961 Wilson, Shenton and Pease submitted to the BNCSR a formal paper and a request to join the Skylark programme.

The Culham approach had, of course, no associated grant application, so in that sense it was extremely welcome, as it was in the technical sense, for Wilson's group was well staffed and motivated. Coming so soon after the important step by the BNCSR, the Culham approach provided just that extra impetus that was needed. The BNCSR next set about drawing up a firm requirement on the RAE. It was not yet clear either at the RAE or within the committees of the BNCSR what references the stabilisation would be against, whether the Sun or Moon, the Earth's magnetic field, the horizon, or an inertial system. I was particularly keen that the RAE should make the leap at once to an inertial reference system, such as was now being employed very successfully in the NASA rocket programme. Whilst in Washington in March 1961 I visited Wallops Island to meet people from Goddard and the US rocket company Aerojet General to learn more about their inertial reference system, based on two three-axis gyroscopes. It seemed eminently usable.

The reaction at RAE to my suggestion was to oppose it strongly. It would have meant spending dollars on gyroscopes rather than pounds sterling in British industry, but that was not the principal objection. The large coning motion of the Skylark rocket that developed as it left the atmosphere caused a major problem; this motion could cause the gyroscopes to topple before head separation and before some degree of control of head motion had been achieved. A sensitive issue was uncovered, Hazell's determination not to attempt spin-up at launch (Chapter 2). One can see with hindsight that had the same positive attitude been taken towards this aerodynamic problem as was later taken in ESRO and at BAC Filton, many benefits would have flowed. But at that stage, early-1961, things were not that obvious. So the RAE preference was to adopt a control system which could survive the coning motion, and to use the Sun, the Moon, the Earth's magnetic field and later the stars themselves. They were discussing with a local firm, Elliot Bros. Ltd (now MSDS Ltd), how the requirement might be met. I had already sat in on a number of meetings, and now, on my return from the Wallops visit, I put forward my argument for gyroscopes. It was clear then what the RAE decision would be. Years later, just before the British Skylark programme was closed down, a full-blooded miniature inertial reference system based on aircraft navigation systems using rate gyros and accelerometers was under development for Skylark. Sadly, it came too late to be used.

This then was the decision at RAE, to use a three-axis, air-jet control system, working to non-inertial references, and to stabilise the rocket head after separation. It was clear that stellar pointing, which the astronomers wanted, would be the most demanding requirement, but the Culham group were now adding powerful support to university groups for, initially, a sun-pointing system. To give the RAE team clear guidance on the priorities a BNCSR subcommittee meeting with them and all interested parties was held in May or June 1961.

Massey was in the chair. Beside him, as always, sat J.F. Hosie, Assistant Secretary at the Office of the Minister of Science, whose task since the latter part of 1960 had been to oversee the developing space science programme on behalf of the Minister, Quintin Hogg, and in particular to arrange financial matters. It was Hosie, a mathematics graduate, who, by attendance at the BNCSR meetings and by consultation with Robins and myself at the SRMU, guided Massey on questions of what might and might not be possible financially, and it was he who negotiated with the Treasury officials the annual space science budget. He sat through an hour or more of tangled discussions by the scientists, growing ever more impatient. They believed they were discussing what they wanted, whereas in fact they were arguing not about "what" but about "how". Being for the most very practical men, the "how" was what interested them all keenly. As a result they could decide nothing. Finally Hosie, the trained administrator, stepped in. 'From what I hear,' he said, 'some of you want to study the Sun, and that's most easily done. Some of you want to look with moderate accuracy at stars, and that's a bit more difficult. Some want to look very accurately at stars, and that is really difficult. Why don't you think in terms of a

staged development programme?’ Then, looking down at a sheet of paper on which he had been busily scribbling notes a few minutes before, he said ‘I’ve made a list of what I think might suit you:

- Stage 1 a simple sun pointer. That should make the Culham group happy because they say that in any case they will add a fine pointing system in their instrument.
- Stage 2 a development of Stage 1 using the Moon as a reference. That will serve as a stepping stone to Stage 3.
- Stage 3 more accurate Sun and Moon sensors, giving sufficient accuracy for some stellar work.
- Stage 4 using horizon scanners. Align the rocket head to the vertical for earth and atmosphere studies.
- Stage 5 full star pointing.’

Hosie’s proposal was immediately accepted. It was short and succinct, it had just the right progressive element, it did not tie the hands of RAE or their contractors, and it was workmanlike. Furthermore it came from someone who had an idea of the likely costs and who knew that he would have to make the necessary provision within the space budget. Hosie’s staging nomenclature stuck; thereafter the stabilisation systems were known by it.

The RAE team, now given the lead they wanted, put their proposals in a paper to the BNCSR in July 1961, retaining the staged programme in exactly the form proposed. Their prompt response was due to the work already put in by RAE and Elliott Bros. With BNCSR approval, a contract for a design study was placed. In May 1962 that study was considered and approval to proceed initially on Stages 1 and 3 given. Stage 2 was not supported because there appeared to be too little scientific interest.

I have described these events in some detail because of the significance that the eventual decision to proceed had for the Skylark programme as a whole. Prior to the decision the programme was in the doldrums. Thereafter it went ahead with new vigour. Many new possibilities were opened up for the experimenters, and a potent new group had joined them. Perhaps as important was the change to a customer/contractor relationship on Skylark between the BNCSR and the Ministry of Aviation, with the placing of the contract with Elliot Bros. The relationship was to grow, eventually leading to the placing of a contract by the Ministry with BAC (Filton) for the construction of all Skylark payloads.

The first MSSL pointed instruments were an X-ray scanning spectroheliograph and a Lyman-alpha detector, flown at Woomera on Skylark 305 on 8 August 1967 for solar observations. In all, MSSL instruments were flown on 19 stabilised Skylark rockets at Woomera. They were often instruments of considerable technical complexity, requiring the investment of a great deal of time, money and ingenuity, yet each returned just a few minutes of data, poor by comparison with satellite instruments. Of the many remarkable payloads, I recall two by Glencross and Brabban which were designed to record line emission from active regions on the Sun. They had built one payload SL 903, flown in December 1971, which used a collimated Bragg crystal spectrometer to define a strip across the solar disc, but they were not able to locate any one source within the strip. They therefore built a second payload, SL 1003, flown on October 1972, with a rotation collimator in place of the fixed collimator with which they located sources across the strip. They also rotated the crystal slowly so as to explore the whole of the observable solar corona.

The method devised at the Royal Aircraft Establishment, where the Skylark system was developed, of building up the payload from a series of body sections clamped together with

manacle rings gave a remarkable freedom to add new sections as required. In time, some extended payloads were to equal the motor in length. Payloads were tested at Woomera before being taken out to the launch tower to be mated with the rocket motor. In flight, the nose cone and motor would be released and the payload would then be pointed towards the Sun or one or more X-ray sources. Parachute recovery was used to return the instrumentation. Later Elliots introduced a control bay in two parts, one valuable enough to be worth recovering, the other not.

HIGH LATITUDE ROCKET CAMPAIGNS

The magnetosphere makes its most direct connection with the Earth's atmosphere and ionosphere at high latitudes, where aurorae are the visible results of energetic electrons and ions impacting on the atmosphere. Auroral heights of several hundred kilometres are readily reached with sounding rockets, and, in the 1970s, campaigns of rocket investigations were organised from time to time to allow direct access to the interaction regions. MSSL staff, working with colleagues from the Rutherford and Appleton Laboratory (RAL) and with other university groups, flew ion and electron detectors in rockets at Andoya and Andenes (Norway), and at Kiruna (Sweden), in 1973-76.

A particular problem with magnetospheric experiments on rockets and satellites lies in deciding whether observed changes in some measured property are of spatial origin, resulting from the fast motion of a craft from one region to another, or are temporal, all regions being affected equally. Simultaneous measurements from two nearby craft can sometimes resolve the matter. Dual launchings of the 2-stage rockets Terrier-Malemute and Nike-Tomahawk in November 1976 at Andenes were made for just this purpose. The payloads were provided by groups in the USA, Norway, Sweden, Austria and the UK (MSSL and RAL).

SMALL ROCKETS

I have made no mention so far of the United Kingdom's small rockets programme, using the Skua and Petrel rockets developed under SRC auspices to provide an economical way of launching small payloads in the UK and the Commonwealth and to meet the needs of groups who found the Woomera-based Skylark launchings too cumbersome. The need for a UK-based launching programme had been in our minds when I was still at the RAE. Hazell and I had managed to obtain backing from F.E. Jones whilst he was still Deputy Director to plan for Skylark launchings from the missile range at Aberporth. We had commissioned the building of a second launching tower and this was set up on a cliff-top site above the range. We had the encouraging support of the range staff, and Boyd's group went as far as to prospect along the shoreline of Cardigan Bay for camera and microphone sites from which grenade explosions might be observed. At RAE we were under the strictest instructions to attempt nothing until the Woomera firings had demonstrated the absolute safety of the Skylark rocket; we would have to demonstrate that the rocket could be absolutely guaranteed to fall within a prescribed area out at sea. When the first in-flight failure of Skylark occurred in 1959 the writing was on the wall and our plans for Aberporth faded to nothing. The launcher was not wasted however; it was dismantled and taken over by ESRO, who used it until a much simpler rail launcher was introduced.

The Skua and Petrel rockets were fired from a military range on South Uist in the Outer Hebrides. Skua 1, a 5-in diameter rocket, was a Meteorological Office rocket sonde, built by Bristol Aerojet Ltd. It released a fine-wire temperature sensor on a parachute. The Skua 2 was developed, with a lengthened body to take a telemetry transmitter. The first was launched in March 1968, and some forty were flown before the termination of the programme in 1972. The MSSL was in the programme early with 12 payloads devoted to positive-ion densities in the D region. Six firings took place at South Uist in 1968.

As part of a Royal Society initiative to foster a Commonwealth research programme, Willmore proposed a complex experiment which had as its scientific aim a comparison of D-region ion densities in the equatorial electrojet region at Thumba, India, and at South Uist, a prime requirement being that measurements be made within two solar-zenith ranges, 45-50 degrees and 65-75 degrees, and at as near the same time as possible. Six Skua payloads were built, the ion probe being a small wire which was coiled inside a small mechanism and deployed through the rocket skin. There was a complication in that the Thumba range used IRIG telemetry so Norman, who was in charge of the project, had to acquire and install three of these non-standard systems. The three South Uist rockets, under Raymont's supervision, were launched in April 1970, two on the 14th and one on the 17th. Willmore, who was to start a six-month sabbatical in India in March, supervised the launching of one Skua at Thumba in February, and Norman two launchings on the 15th and 16th of April. The effort was not a success scientifically. The telemetry failed on one Thumba launch, and two of the South Uist flights were not sufficiently successful to yield the necessary quality of data. We really were trying to run before we could walk.

The Petrel rocket was a 7.5-in diameter vehicle, again designed and built by Bristol Aerojet Ltd, under an SRC contract. It used an end-burning Lapwing solid-propellant motor developed at the Rocket Propulsion Establishment, where the Skylark's Raven motor was developed and filled. The project was operated by the Atomic Weapons Research Establishment, who provided a complete user service. Between 1968 and the termination of the programme in 1982 some 234 Petrels were launched, about two-thirds of them from South Uist. We at MSSL made only limited use of Petrel, two ionospheric payloads flown in October 1969 and two more in September 1970. The new suprathermal analyser was flown during an auroral event from Andoya in October 1972, and in September and December 1974 Keith Norman flew two more of the short path-length mass spectrometers (Chapter 6) from South Uist.

CASTING YOUR BREAD UPON THE WATERS

Len Culhane established a close collaboration with the Lockheed Space and Missile Corporation group, spending six months with them in 1970. Many valuable consequences stemmed from that visit and from his continuing association with the space group there. One in particular that I recall was the opportunity to construct a one-dimensional X-ray position-sensitive detector, using a resistive-wire counter, for Lockheed's crystal spectrometer. It did not appear on our list of SRC grant-supported projects, but neither did it cost very much, and so it was, as it were, smuggled in. In any case, we were collaborating with Lockheed in building a crystal spectrometer for Skylark 1302, so there was a quid pro quo. Chris Rapley took on the task of developing the counter and of taking it to Lockheed for integration. In due course the instrument was flown on an Aerobee rocket and many spectral lines were recorded. A much greater pay-off, however, was that a year later, as a member of a collaboration with Lockheed and the Appleton Laboratory chosen to provide the X-ray polychromator for NASA's Solar Maximum Mission, MSSL contributed amongst much else eight such resistive-wire counters for the bent crystal spectrometer. Those counters, whose development began so unobtrusively, helped us to play a key role in the largest and in many ways the most successful project yet.

ULTRAVIOLET ASTRONOMY

Boyd's space research group had been the first in the UK to attempt measurements of ultraviolet emissions from stars. Heddle built a rocket payload consisting of five photomultipliers, sensitive to ultraviolet light, viewing the sky over a range of angles through honeycomb collimators. The instrument was flown on Skylark SL43 on 1 May 1961 at Woomera. Radiation was detected from 22 stars, a notable first, but it was clear that further progress would have to await a head stabilisation system. By 1965 the group had an ultraviolet instrument selected for NASA's OSO 6 spacecraft. That was successfully launched in August 1969, and the instrument, which Bruce Woodgate managed, did its job well.

We saw in Chapter 10 how in 1960/61 the Astrophysics Research Unit came into being at the Culham Laboratory of the UK Atomic Energy Authority with Bob Wilson, now Sir Robert Wilson FRS, Head of the Department of Physics and Astronomy, as its leader. When the first gas-jet pointing systems came into operation from August 1964, he and the ARU proceeded to participate in the UK Skylark high altitude rocket programme.

In 1968 the UKAEA decided that the ARU at Culham could no longer be supported. There was an outcry, letters to 'Nature', one from as far away as the USA, and a move by Boyd, Sayers, Elliot and others to persuade the SRC to 'rescue' the unit. The move succeeded, and on 1 April 1969 the ARU, though remaining at Culham, became an SRC unit. Early in 1972 it was formally incorporated into the Radio and Space Research Station at Slough as the Astrophysics Research Division though still remaining at Culham. The proponents of the rescue package had made the proviso that after three years the unit should have found a home in a university, but it suited the SRC very well to have the ARU remain where it was, adding lustre to its name.

The considerable experience in ultraviolet astronomy which was built up at Culham under Wilson's leadership led to a series of developments involving two ESRO satellite projects and a joint NASA/ESA satellite project. These, in the sense that they involved the MSSSL in only the most marginal way if at all, have no real place in these reminiscences. However they did involve Wilson, Boksenberg and UCL closely and so deserve some mention, if only to provide a little historical background to MSSSL's activities.

The first of the projects was TD 1, one of two small stabilised satellites which formed part of ESRO's early programme. They were to be launched on the US Thor-Delta rocket system, hence the name. The Royal Observatory, Edinburgh, and the University of Liège had separately proposed similar ultraviolet astronomical experiments, and these became merged as the Edinburgh-Liège proposal. It called for a 25-cm Cassegrain telescope. The proposal was accepted for TD 1 in October 1964, together with six other smaller experiments. At once the complexities and huge cost of carrying out the main experiment became apparent. As we saw in Chapter 9, the second project, TD 2, was cancelled because of the mounting costs, allowing TD 1 to go ahead. One complexity was the need for a sun baffle, for the design of which ESRO placed a contract with a small company formed by members of the UCL physics department, with Boksenberg the chairman. Their work on the baffle proved entirely satisfactory.

By early-1968 - which gives some measure of the financial, organisational and technical difficulties being faced by ESRO - progress in the UK on its part of the main instrument in TD 1 was so far behind schedule that the SRC brought in Wilson to lead an RAL group, backed by Boksenberg at College, to take full charge of the payload development. In the original design of

the ultraviolet spectrometer, three or four spectral lines were to be measured in one of its two channels, the other being a single photometric channel. Boksenberg made a valuable contribution by pointing out that if the entrance slit of the spectrometer were to be opened up the motion of the sun-synchronous orbit proposed for TD 1 could be used to scan a stellar image across the slit, so giving a great deal more spectral information at the detector.

Wilson revised the estimate of cost of what had now become a national project, first to £400,000, then £800,000, and later still £1,172,000. In January 1991 prices, that would be about £13,000,000. The Space Policy and Grants Committee of the SRC was in despair. It would not for one moment have considered the project worth such a price but the fear of international repercussions were it to back out compelled it to find the money. With this huge increase in investment of staff and money, and with Wilson's leadership, the SRC ensured that the TD 1 instrument was successful. There was a good launch on 9 March 1972 into a polar sun-synchronous orbit. There was three-axis stabilisation with one lateral axis pointing continuously at the Sun, and the optical axis scanning a great circle on the sky once per orbit. The motion of the Earth around the Sun then completed the required sky survey in six months. This same orbit was used by ESA for its infrared astronomy satellite IRAS in January 1983. The results from TD 1 were eventually published in the Catalogue of Stellar Ultraviolet Fluxes covering over 31,000 stars.

It is perhaps surprising that, throughout those initial years of the TD 1 project, with ESRO's difficulties being no less than the UK's, a quite separate astronomical satellite project was being planned in ESRO, the Large Astronomical Satellite, LAS. The first steps were made in 1963. The preliminary scientific aim was, again, ultraviolet astronomy, but at very high spectral resolution, about 1Å. The secondary aim was broad-band X-ray observations, and in this we may confidently see Boyd's hand. Massey was determined that the UK should have a major role in the LAS. In 1964, when Wilson's group at Culham was concerned only with rocketry and the TD 1 task, and the SRC rescue of the ARU still lay in the future, Massey brought together Wilson and J.B. Adams, the Director of the Culham Laboratory, Boyd and Boksenberg from UCL, and Ieuan Maddock of AWRE, who had been of such help with Ariel 1 data handling and who had a professional interest in the ultraviolet. They agreed to prepare a proposal to ESRO for an ultraviolet spectrometer and an X-ray instrument to be carried on the LAS. Wilson became the chairman of the UK group.

The LAS proposal was submitted in December 1964, together with two others from European consortia. ESRO placed contracts with the three groups to produce detailed design studies by 31 January 1966. In May 1966 the UK design was adopted, but as an ESRO project at ESLAB. Here there lay difficulties, since Wilson could not transfer to the staff of ESLAB. Whilst that problem was being talked over, the inevitable financial crisis appeared, triggered by the TD projects which were now seen to be likely to cost a great deal more than had been foreseen. The UK offered to work on the development of its proposed instruments throughout 1967, with reimbursement postponed until 1968, but ESRO was not tempted. Unable to see how it could fund the instrumentation packages, ESRO nevertheless decided in a rather muddled way and by only a majority vote, which did not include the UK, that it should get on and design the spacecraft that would one day carry the Culham-designed instruments. In March 1967 W.G. Stroud of NASA was brought in to head a group preparing and costing a Project Development Plan. In June 1967 he presented his estimates - between £31.5m and £40m for two models of the LAS and a further £14m for ground support. These figures finally put an end to LAS, though the project had been dead for all real purposes since the end of 1966. The UK priced the instruments at £2.6m and £165k for the ultraviolet and X-ray instruments respectively.

ESRO's decision to abandon the LAS was a most disappointing outcome for the UK team, but Wilson was convinced that the cost of the whole spacecraft package could be reduced

considerably by designing the ultraviolet instrument and the spacecraft as one entity. He brought together the instrument group again and with the collaboration of two industrial firms it set up a Project Study Group. The result was a proposal for an Ultraviolet Astronomy Satellite, UVAS. Boksenberg played a vital role in UVAS and its subsequent history, but, with no X-ray element in it, the association of the MSSL through Boyd ceased.

UVAS was liked by ESRO's Advisory Committee but even so ESRO turned it down on financial grounds. Still undeterred, Wilson approached Leo Goldberg, then Chairman of the US National Academy of Science's Space Science Board. The Board passed the proposal to the Director, GSFC, who arranged for a detailed evaluation. The upshot was that NASA adopted the project as a joint one with the UK, to be part of its Small Astronomical Satellites programme. An operational plan was worked out. Following a NASA suggestion, it was agreed that the satellite would be put into a figure-of-eight geosynchronous orbit in such a way that it would rise over the horizon for 8 hours each day as seen from a European ground station and for 16 hours a day as seen from a station at Goddard. Funds to support the work in the UK were first granted in 1970. Later ESRO was invited to join, and it did. Boksenberg became UK project scientist.

In 1972 Wilson left the ARU, now of course a unit of the SRC, to take the UCL Perrin Chair in Astronomy that had been vacated by C. W. Allen on his retirement. At about the same time the two departments of Physics and Astronomy merged into a single department. In 1973/74 the name of the SAS project was changed to the International Ultraviolet Explorer. In 1974/75 acute difficulties arose with the UK detector system. Boksenberg was by this time deeply involved in a quite new and exciting venture, the Image Photon Counting System, and was unable to devote sufficient time to his IUE responsibilities. In May 1975, therefore, Wilson was appointed project director, nominally on a half-time basis. Thus for the second time he was called upon to assume the management of an ultraviolet astronomy project part way through, and in both instances he did so with great success. The spacecraft was launched on 12 January 1978, and continues to operate with outstanding reliability in its fourteenth year.

ARIEL 6 - THE LAST OF THE BREED

Ariel 6 was the last of the United Kingdom's small scientific satellites. That the end should come was obvious by the early-1970s. The space scientists' aims were becoming ever more ambitious and therefore expensive, beyond what the SERC's budget could afford, and it was to the ESA and NASA programmes that the UK scientists were encouraged to look for future flight opportunities. There were growing calls on the resources of the SERC's Astronomy, Space and Radio Board. The optical astronomers, having achieved a stake in the Anglo-Australian telescope, were planning a bigger development, the removal of the Isaac Newton telescope to Las Palmas and the building of a new 4-metre telescope, the William Hershel telescope, at the same site. The infrared astronomers were working on a new light-weight telescope to be used at Mauna Kea. The ultraviolet astronomers' participation in the NASA/ESA International Ultraviolet Explorer had to be funded. The radio astronomers at Cambridge and Jodrell Bank were planning large new installations. The gamma-ray astronomers were badly disappointed by their exclusion from the ESA Cos-B project. But the twists and turns in the fortunes of the various contending activities left a window of opportunity for the astronomers. In 1972 the ASR Board approved one more Ariel project, and the lot fell on the gamma-ray astronomers. Peter Fowler, Professor of Physics at Bristol, was chosen to provide a detector of heavy cosmic ray primary particles.

Peter Fowler, son of Sir Ralph Fowler and grandson of Rutherford, had been in the Royal Air Force during the war as a young, conscripted signals officer. Whilst working at a 'Gee' radar station he distinguished himself by a characteristic piece of resourcefulness. Using his and another 'Gee' station, he located and reported the position of a newly-activated German transmitter that was being set up to jam the 'Gee' system. Having been located, it was promptly destroyed. Back at Bristol to complete his degree, Fowler infiltrated Powell's cosmic ray group in his third undergraduate year and remained with it for the rest of his career. I knew him as an ebullient, confident fellow undergraduate. One of the pleasures of meeting him since has been to find him always so little changed.

The Bristol cosmic ray detector proposed for UK 6 was so large that it first seemed to deny space to any other experimenter. However, ingenuity prevailed and means were found to strap to the exterior of the satellite body two pairs of cosmic X-ray detectors. One pair, to be built by Ken Pounds' group at the University of Leicester, was to look for rapidly-fluctuating X-ray sources. The other pair, to be built jointly by the MSSL and the University of Birmingham, was to observe X-ray sources at longer wavelengths than had been attempted previously; the observations would be technically more difficult to make, but the information they were expected to provide on the physical conditions at the source and in gas clouds would be of particular value. Both pairs of X-ray instruments were to look forwards along the spin axis, which had therefore to be controlled with some precision.

The arrangements chosen by the SERC for the management and construction of the satellite were much the same as those for Ariel 5, the same clumsy partnership between contractor, Ministry of Defence, the Appleton Laboratory and the SERC, the same diffuse spread of responsibility. Worst, the same derogation of the experimenters' position to that of a tiresome complication without which the project would have been so straightforward.

The MSSL instruments used parabolic reflectors or “light buckets” to capture as many soft X-ray photons as possible from narrowly-defined areas of sky and to concentrate them on novel propane-filled X-ray detectors. Improved data analysis on the ground allied to the more advanced data-handling electronics in the satellite was expected to give a much higher wavelength resolution across the soft X-ray band. Previously our hope had been to build an imaging system with a secondary reflector and a position-sensitive detector, but the detector technology was still a few years from fruition, and optical alignment techniques for the assembly and testing of an imaging X-ray telescope would have had to be developed. It would have been a bridge too far. So, in partnership with the Birmingham group, we retrenched and settled for the non-imaging soft X-ray experiment, working at 1.5-0.1 keV, or 8 Å-120 Å. It was important to collect as many photons as possible, so two reflectors and detectors were incorporated in each strap-on unit. The project scientist at the MSSL was Mike Cruise. Franks at NPL undertook the finishing of the reflectors.

To ensure reasonable transparency the low-energy detector windows were to be of very thin plastic supported on a honeycomb mesh. Earlier experience with various grades of plastic for detectors on rocket payloads had taught us that a thin plastic film is permeable to the detector gas, so that in a satellite operation continuing over years a gas replenishment system would be needed. A detector is normally filled with a mixture of a rare gas, such as argon, and a quenching agent, such as methane. If the detector is to retain its calibration both the pressure and the gas ratio must be held within specific tolerances. But the two gases leak through plastic at different rates and so the replenishment system would have to have separate reservoirs of the two gases, be capable of sensing the gas ratio in the counter, and then of making up the loss in such a way as to ensure the correct gas ratio. This was a tall order, and eventually it was decided to make a radical change and adopt a single gas, propane, which acts as its own quenching agent.

The design of the gas system became Sheather’s responsibility. He set up an experimental rig and showed that it was possible to maintain the propane density in the two counters in each unit with the required precision, and, with a propane gas bottle small enough to be squeezed into the very limited space available, and to do so against the worst-case leakage rate for the required life time. The particle background in orbit threatened to put a serious limit on sensitivity and so a complex anti-coincidence scheme was employed, so successfully that Cruise was confident that the particle background would not be the limiting factor. In the event the scheme worked well except in the heart of the radiation belts, so after launch command arrangements had to be made to switch off the detectors as the danger point was neared.

The use of reflection optics to give a well-collimated view of the sky brought with it a problem that we had previously not encountered, that of controlling and determining the spacecraft’s pointing direction with sufficient accuracy. It was a challenging task, undertaken for the experimenters by the SERC’s Appleton Laboratory. Attitude control was by gas jets and magneto-torquer. A number of ‘horizon-detector’ attitude sensors were employed.

A particular feature of the project was the extensive calibration of the two instruments, which was carried out at Birmingham. This was of particular importance later in the project when it became clear that the mirror reflectivities had changed after launch. The completed instruments, securely cocooned in their purpose-built transit cases were in constant motion from site to site, Holmbury, Birmingham, Portsmouth, Farnborough. One afternoon a very tired Mike Cruise returning to Holmbury by car with the instruments in the back, slid off the road only half a mile from the laboratory, fortunately without hurt. The transit cases proved their worth, and only minor damage was sustained by one instrument.

The launch site was on Wallops Island on the Delmarva peninsula, remarkably close to a populated area. One could not but contrast it with Woomera, a fiendishly remote and expensive range which had been sited there in the almost paranoid days when security and the Russian threat dominated all other considerations. The first launching from Wallops in the Ariel series had been of Ariel 2, of which I was UK project Manager, on a Scout four-stage solid-propellant rocket. Following the failure to have Scout ready for the Ariel 1 launch there was much concern to know whether it would be ready for Ariel 2, and, if so, what its performance would be. At each joint S 52 working-group meeting a representative from the Langley Air Force Base, near Wallops, where the Scout development team were located, gave a progress report. For some while his reports were depressing; either we should have to lighten the payload or accept a lower orbit. Both alternatives were equally unacceptable. Then came a ray of light. There had been a trial, he reported, and it showed that everything would be fine; we could have our full payload weight and our orbit. 'So has there been a Scout firing,' we asked. 'Oh no,' said this genius, 'we did the trial on the computer.'

The Langley Air Force Base was the scene of an amusing incident in October 1962, at the height of the Cuba crisis, when the S 52 team were flown on a NASA shuttle from Washington to Wallops for a progress meeting, landing at Langley en route. The airfield was prepared for war. Lines of Boeing B-47 jet bombers and Super Sabre fighters were at the ready. As we sat on the airfield awaiting a passenger, Roger Jennison and one of his team from Jodrell Bank jumped out, seizing the chance to photograph this aerial armada. As they stood there happily snapping away they were descended upon by a fleet of military police. Bad enough to have people photographing inside the airfield, but that they should be foreigners was appalling. However, there were apologies all round, the cameras were opened and the films confiscated, and we went on our way.

Ariel 6 was launched from the Wallops range on 2 June 1979 into an excellent orbit with all its scientific equipment working well. However it was plagued throughout by interference from a powerful ground-based radar, believed to be Russian, which repeatedly triggered the satellite command system, switching off the Leicester and the MSSL/Birmingham X-ray instruments. The Bristol cosmic ray experiment relied to only a very minor extent on ground commands and so was largely immune. This interference was not found to be affecting other spacecraft then in orbit and the conclusion forced upon us was that faulty design or fabrication somewhere in the spacecraft's command system had left it vulnerable. Whatever the cause, the urgent question that followed was - 'what additional ground commands could be sent to the spacecraft?'

A plot of the switch-off times and positions made it clear where around the world the trouble originated and where the transmission of an 'on' command on certain orbits each day would be most beneficial. NASA was cooperative, but at a cost and for a limited period because of other commitments that soon required the services of their ground-command stations. Consequently a remarkable piece of MSSL initiative came into play, thanks mainly to the skill and enterprise of Phil Guttridge and Barry Hancock. Between them they drew up plans to build an automatic command transmitter to be put in the care of Ian Tuohy, one-time research assistant at MSSL, now on the staff of the University of Canberra, Australia. Guttridge, with his experience as a radio amateur, ordered a receiver, and two ground transmitters built to his specifications. Between them they designed an aerial, a rotating mount with command control, and a computerised control system which accepted a week's predictions in time and azimuth for the appearances of Ariel 6 over the horizon. There were usually three appearances each day on each of the northbound and the southbound transits. Shortly in advance of each transit the computer brought the rest of the system alive and swung the aerial to the appropriate azimuth angle. On receipt of the spacecraft's signals - it being the X-ray instruments that were off, not the telemetry system - the transmitter sent an 'on' command, listened for the routine

acknowledgement, and re-transmitted the command twice more for good measure. Then the whole system closed down until the next programmed interception. The construction and testing of the station was completed at great speed. Guttridge and Hancock took it out to Canberra, set it up and came home. With Tuohy watching over it, and supplied with forecasts that were telexed to him weekly, the commanding system continued to do its job until the termination of the project.

Once the satellite had been placed in orbit the matter of attitude determination and control became of first importance to us. Unfortunately the spacecraft ran hotter in orbit than the design aim and the high-accuracy horizon sensors were put out of action. Faulty software used in real-time attitude reductions led to incorrect attitude trims. Obtaining satisfactory pointing and subsequent attitude solutions to meet our needs thereafter proved a taxing task for the Appleton team, and it was to be four years after the measurements were taken that sufficiently reliable solutions were available for the measurements on point sources to be interpreted with any confidence.

Ariel 6 was turned off for the last time in February 1982. Despite the invaluable help of the MSSL command system, the project yielded far less data for the three X-ray astronomy groups, MSSL, Birmingham and Leicester, than its potential had promised. Even so, many sources were observed at the uniquely long X-ray wavelengths of our instruments and much information was also obtained on the diffuse soft X-ray background.

13

MORE ESA SATELLITES

GIOTTO

Giotto, ESA's mission to comet Halley, was the dark horse in the ESA stable which nearly never was. ESA has a lengthy process for selecting each major project. Sometime in the 1970s I attended one of the open meetings in Paris at which forceful presentations were made on behalf of a number of hopeful consortia. One proposal, which I had no hesitation in consigning mentally to the dustbin, was fronted by an Italian. He proposed sending a probe to intercept Comet Halley at its next apparition. The scientific case seemed unexciting, certainly when set against the astronomers' ambitions. What lodged most forcefully in my memory was the final appeal to the large assembly. 'Don't think about the science, think of the publicity value to ESA. Just imagine the impact on the man in the street. Think of what it would mean to him, a European probe being sent to meet a comet!' It was audacious stuff, which most there seemed to dismiss out of hand. Certainly I did. But in the end that was ESA's choice, and yet it was not even short-listed for the meeting of the Science Advisory Committee, on whom the responsibility for a final recommendation to the ESA Council rested. Quite how the volte face occurred I cannot say.

ESA made its decisions in 1981 on the instruments to be carried. Amongst them was the package proposed by an international consortium led by Johnstone at MSSL, its purpose being to measure ions from two sources, the solar wind and the comet itself, so as to obtain information on ion interaction and flow around the comet and its plasma tail. The instrument consisted of 3 units, the Fast Ion Sensor (MSSL), the Implanted Ion Sensor (Lindau, West Germany) and the Data Processing Unit (Frascati, Italy). The project team, which was led by MSSL, also included groups at Kiruna (Sweden), the Southwest Research Institute (Texas), the US Los Alamos Scientific Laboratory, the RAL, the University of Cardiff and the Max Planck Institute, Garching (West Germany). The principal hardware builders were, of course, MSSL, the Max-Planck Institut für Aeronomie at Lindau, and the Instituto di Fisica dello Spazio Interplanetario at Frascati. We saw much of those two groups as the testing and integration of the instruments package proceeded.

The launch by an Ariane 1 rocket from Kourou, French Guiana, on 2 July 1985 was faultless. The instruments were tested in August and September during the cruise phase during which the whole satellite was checked out and the instruments collected valuable solar-wind data. Giotto was directed to an excellent encounter course which took it 600 km from the comet's nucleus on 13/14 March 1986. Data were received throughout the entire passage through the coma apart from a 30-second break at closest encounter, and the mission objectives were fully achieved.

The encounter was given full TV coverage, as the proposer had foreseen, and the fact that the payload included a camera helped enormously. The pictures were extremely impressive when tidied up and made into an encounter sequence. They would have been even more impressive had the camera team been aware that the body of the comet was black, not bright. The camera had been programmed to view a bright object and in the event the brightest area was the scattered sunlight in the plume or plumes of gas being emitted from the comet's surface. As a consequence, the closer the approach the more the camera pointed away from the comet itself and towards the bright plumes.

HIPPARCOS

When ESA chose the HIPPARCOS astrometry spacecraft as a major project it contracted with two European consortia to make scientific-instrument proposals. Cruise at MSSL joined one of these consortia, but to his disappointment his was not successful. He later saw the rival proposal and was full of praise for it.

Having lost any role on the instrumentation side, Cruise in 1982 put to use the great deal he had learned about the astrometry problem faced by the HIPPARCOS project by joining one of two groups formed to handle the data analysis, the Northern Data Analysis Consortium. This group was already active in preparing plans to generate the software. With help from Jeremy Allington-Smith, algorithms were written for the analysis of the raw photon counts from the main detectors and the star mappers. These algorithms were then incorporated into RGO software, and that stage was reached in 1985. Cruise also participated in the work of a small group on the question of incorporating relativistic corrections into the analysis. As the simulations with the software packages progressed the results were compared with those by the FAST consortium, (Fundamental Astronomy using Space Techniques), and this proved of great value. Our rôle in the project ended in 1987. HIPPARCOS was launched in 1989 but not into its correct orbit. At first it appeared that the project might fail but there has been increasing optimism that good results will ultimately appear.

THE SOLAR MAXIMUM MISSION

The NASA Solar Maximum Mission (SMM) employed a very large, fully-stabilised spacecraft carrying seven instruments for solar observation, six of which were spectroscopic and designed specifically for solar-flare observations over a wide spectral range. MSSL, in collaboration with the RAL and the US Lockheed Palo Alto Research Laboratories, contributed an X-ray polychromator which played a vital part in the SMM concept, observing soft X-rays between 1.8 Å and 23 Å with two complementary instruments. One was the Bent Crystal Spectrometer which formed high-resolution spectra in limited ranges near 1.9 Å and 3.1 Å for observations of spectral lines of highly-ionised iron and calcium emanating from solar flares. The other was the Flat Crystal Spectrometer which provided high-resolution images of those flares at six X-ray wavelengths. The instruments were complementary in that the BCS gave high time and spectral resolution at the expense of spatial resolution, its collimator giving it a field of 6x6 arcmin, whereas the FCS gave high spectral resolution and good spatial resolution with a field of 12x12 arcsec, at the expense of time resolution. The 1-metre-long collimator of the FCS could be rastered across the solar disc in 5 arcsec steps over a chosen area of 7x7 arcmin to build up an image of a flare region.

The joint proposal, by three groups with considerable prior experience in the field of solar X-ray and ultraviolet astronomy, was first accepted by NASA for consideration and management studies in 1975. Early in 1976 the proposal was formally accepted, and work got under way at the three laboratories, with a launch at the end of 1979 in prospect. At MSSL we had a share in the design, manufacture and testing of both instruments, with Chris Rapley leading the work. The spacecraft itself was very large, and the polychromator the largest instrument package with which we had been associated.

The detectors were our particular responsibility, the sealed position-sensitive detectors in the BCS having stemmed from a small rocket project carried out jointly with the Lockheed group (Chapter 10). The detectors in the FCS were of both the sealed, beryllium-windowed variety and the unsealed thin poly-propylene-windowed type needing the same kind of active control and replenishment propane system as had been developed for Ariel 6 (Chapter 11). The need to be able to test the detectors in a vacuum against a source of the smallest possible angular size led to the setting up of an all-too-short “long-beam” X-ray facility running between two small wooden huts. These were later displaced to make way for the Boyd Remote Sensing Centre. As the project progressed, the centre of activity, particularly for Rapley and his family, and Bob Bentley, moved first to Palo Alto and then to the GSFC at Greenbelt, Maryland.

SMM, as well as the British contribution to it, was clearly a well-planned and efficiently-run project for the huge spacecraft was launched very successfully in February 1980, within only two or three months of the date declared in 1976. There was, of course, every pressure not to miss the solar maximum. All instruments functioned extremely well - a major success and a triumph for the teams at GSFC and for the many experimenters. It was a triumph, that is, until November 1980 when control of the spacecraft was lost and it began to tumble in orbit. A fault had occurred in the spacecraft's inertial reference system. One by one the four gyroscopes lost power. There had been an oversight at GSFC in the design of the power supply, in particular in the choice of fuses. These were known to degrade in use at a rate dependent on the current carried, and so were initially specified at a sufficiently high rating to make allowance for

degradation. Some time after the design had been laid out and approved an extra electrical drain was placed on the fuses, without the consequences being appreciated. After launch the fuses degraded so rapidly that instead of seeing out the planned lifetime they failed in ten months. By good fortune it still proved possible to stop the tumbling and to bring the spacecraft under control in such an attitude that adequate power was being generated and no further damage was being done.

At once discussion began about a Space Shuttle rescue mission. Could the very busy schedule permit a visit to SMM, a space walk, and the replacement of the faulty units? SMM was the first spacecraft to have been built to allow in-orbit replacement of instruments, and so the job was feasible. In planning the exercise, much care had to be exercised to ensure the safety of the astronauts. The presence of two propane gas cylinders in the polychromator was at first thought to be too dangerous and there was talk of having to dump the gas and do without the detectors that used it. But after a visit to Holmbury by a GSFC engineer the gas supply was accepted as safe. The opportunity was taken to arrange for a cover to be placed by the astronaut over an aperture in the side of the spacecraft where plasma had been leaking in and causing problems with the FCS power supplies. In 1982 the decision to repair was taken, and a Space Shuttle flight in 1984 was named.

The SMM repair was carried out by the crew of the Space Shuttle Challenger in April 1984. The spacecraft was first captured with the mechanical arm and then brought aboard and secured to a purpose-built workstand in the bay. There, James van Hoften and George Nelson replaced the faulty attitude-control module and carried out the various refurbishment tasks, including the difficult job of replacing a faulty electronics box which had halted the coronagraph/polarimeter. A baffle was duly placed over the troublesome hole. The spacecraft was then put back into its own orbit. Much of the operation was seen the world over on television.

SMM had by then been in orbit for four years and it was inevitable that there should be some instrumental degradation that could not be corrected in orbit. The X-ray polychromator group found that they had lost one detector on the FCS and two on the BCS, three detectors out of a total of fifteen. Fortunately, or perhaps it would be more correct to say by good design, there had been overlaps in the spectral channels, two out of three of the lost channels being covered. Otherwise all systems in the polychromator were working normally. Anxiety about one of the two redundant FCS crystal-drive mechanisms remained. A small bulb in the encoder read-out of one had failed prematurely - the bulb, it turned out, had been from a faulty batch. The second drive was brought into use, but somewhat apprehensively as there was no way of knowing whether the bulb on it too would fail early. So two resources had to be treated conservatively, by carefully monitoring the propane gas supply and the crystal drive, the latter requiring there to be much less wavelength scanning on the FCS. In this way it was hoped that at least a year's more life would be possible, probably more. There was much satisfaction amongst the consortium that had designed and built the polychromator that it had survived so well.

Observations were resumed after the April 1984 rescue mission and continued not for one year but for over five years, most successfully. By the autumn of 1988 mechanical problems with the crystal drive had restricted its use. In May 1989 one of the two propane bottles became exhausted, losing us two FCS channels and leaving only one counter operational.

In July 1989, almost nine and a half years after launch, nine of the fifteen detectors were still working, a remarkable record. More than 50,000 orbits had been completed and the measurements had extended over most of a solar cycle. But the altitude of the spacecraft was falling rapidly. Attempts to arrange another Space Shuttle visit and a boost to a higher altitude were made but most agreed that it would not be justified. Re-entry was not far away - it occurred at the end of 1989.

TWO SPACELAB EXPERIMENTS

With the Apollo programme in the USA ending in 1972, NASA began work on its ambitious 'Post-Apollo' programme. In 1969 the USA invited participation in three parts of the programme, the Space Shuttle, a permanent space station, and Spacelab.

At the beginning of June 1970 Sanford and I attended a major 2-day presentation to ESRO in Paris by NASA on its plans for the space station. It was to be operational in the 1980s, serviced by the Space Shuttle and manned by between 12 and 50 astronauts. It was an impressive, well-staged presentation and it carried conviction, so much so that a party of senior administrators and European space scientists, including Massey, laid plans to have follow-up meetings with NASA in Washington and later at the Ames Centre in California. There was a feeling amongst the British that just as the announcement to COSPAR in March 1959 by NASA, six months after its creation, was a turning point for UK space science, so this invitation to ESRO to participate in the space station would lead to a significant change in European space science.

But ESRO was in no position to cement such an arrangement, which would have called upon the Europeans to fund perhaps ten per cent of the space station costs. There was anxiety amongst the French who wanted an independent launcher system developed in Europe and saw ESRO participation in the space station as inevitably drawing funds away from that possibility. At the same time they did not want to see Europe committed to dependence on NASA launchers on which restrictive conditions might be placed, particularly for commercial launches. The British, on the other hand, preferred to avoid the costs of duplicating the US rocket work and felt that NASA could be relied upon to supply rockets for all reasonable purposes.

Europe's difficulties were compounded by the European Launcher Development Organisation's approaching collapse for technical and financial reasons. With a British first stage, which always worked, a French second stage which usually did and a German third stage which often didn't, there was much room for discord within ELDO and the countries which supported it. Indeed Britain had already withdrawn at the end of 1969, being opposed to launcher duplication with the USA. Now other countries were proposing to transfer ESRO money to ELDO to make up for our departure. ESRO's future was under severe threat in other ways. France and Denmark denounced the convention and threatened to withdraw at the end of 1971. Space scientists, amongst them the British, were widely dissatisfied with ESRO's poor value for money, and pressed for withdrawal in favour of bi-lateral cooperation with NASA.

The upshot of this widespread disagreement within Europe was perhaps surprising, for it had seemed to the scientists very much on the cards that both ELDO and ESRO would disappear. Successive visits of British ministers to European Space Conferences - and there seems in retrospect to have been a different minister at each visit - were notable for their lukewarm approach. However by 1972, at the fifth in the series of Conferences, the various European Ministers agreed on a four-point plan, the creation of a European Space Agency out of ESRO and ELDO, the adoption of the French proposal for a European launcher, Ariane, to be developed by the new agency, the setting up of a satellite applications programme, and the development of Spacelab in cooperation with NASA. The agreement was a feather in the cap for the French who got everything they really wanted at the cost of agreeing to Spacelab. It was to be another three years (30 May 1975) before the ESA convention was signed by Ministers, and

another five before it was fully ratified, but December 1972 was the turning point. My recollection of events at the 1972 meeting of ministers was that the British agreement was a political decision in which the argument about the level of space activity in Europe came low on Mr Heseltine's list of priorities and was probably a kind of bargaining counter.

Between 1970 when the NASA offer to ESRO was made and 1972 when this new plan for Europe was agreed in principle, the confusion that reigned in Europe made it clear that there could be no prospect of agreement with NASA on the space station. Their costings had increased and their offers had reduced until one project only remained, Spacelab, on which ESRO had in one sense jumped the gun and begun studies. This may have led to the survival of Spacelab in the agreement, though why remains for me a puzzle.

Two facts faced the newly-created European Space Agency, one that it had no fat in its budget, and the other that, as ESRO, it had been constantly criticised by the space science community for giving bad value for money. Nevertheless here it was recommending, and getting ministerial approval for, a project which, through its association with man in space, was bound to increase greatly in cost, and would only give the scientific community an illusion of more flight opportunities. Boyd and I were at one in deploring any connection by ESA with man in space because of the severe penalties that would come in its train.

On 1 January 1979 the space science committee of the European Science Foundation came into existence. (Prior to that time it had been an ad hoc committee under the chairmanship of Massey; I was its acting secretary.) In September 1979 it prepared a document for consideration by the Executive Council of the ESF entitled 'Space Science in Europe', one paragraph of which read:

'The weakest link in the development of European space research is the lack of adequate flight opportunities. We have to achieve a launch frequency of about one satellite per year, fully instrumented by Europe, to develop our areas of excellence. The expectation that Spacelab would help in this context has not been fulfilled because of its high cost. Therefore a significant increase in ESA's funds for scientific projects is requested. At the same time ESA should strive to achieve a more economic use of its funds. We argue that there is now in fact an opportunity to recover the setback in the scientific programme incurred in 1972 and to improve the situation beyond this because the large expenses required for the development of Spacelab and Ariane are coming to an end.'

This statement airs the fear that many had earlier about the decision to include Spacelab in ESA's list of agreed objectives. But it was a fait accompli, and for us at MSSL not to have participated would have been willingly to cut the laboratory off from experiment opportunities and funding. Consequently Boyd encouraged the submission of proposals for experiments to be flown on Spacelab, in what was clearly to be an entirely novel environment and with quite different ground-support arrangements.

Two proposals with which we were associated were accepted. Thereafter we began to learn the reality behind the rash claims by the proponents of the Space Shuttle and Spacelab in the USA. NASA had once claimed that laboratory-type equipment would be taken aboard from University groups, operated in orbit in a 'shirt-sleeves' environment, possibly by the experimenters themselves, and then returned. It became known as a 'ship-and-shoot' service. We were utterly sceptical, and rightly so. It went diametrically opposite all we had learned about flying equipment in space, and it is extraordinary to think that NASA made such fallacious claims. We knew that the presence of men aboard Spacelab and the Space Shuttle would make our choice of suitable equipment even more restrictive than for a conventional spacecraft, and we also knew

that equipment would have to be purpose-built to the same high standards as for conventional satellite research if only because of the launch vibration.

SPACELAB 1 GAS SCINTILLATION COUNTER

The Exosat spacecraft, it will be recalled, carried as an 'add-on' experiment a gas scintillation X-ray proportional counter, promising an improvement in energy resolution over a conventional gas proportional counter of a factor of two or three. The counter was designed and built by a consortium made up of groups from the Space Science Division at ESTEC and the Universities of Milan and Palermo, and by MSSL. This same design of instrument was also proposed for the first Spacelab flight by the same consortium and was accepted in 1977 for the observation of the brighter cosmic X-ray sources. A ground check-out system was built for it, to be moved with the equipment to the USA for the final check-out and the orbital operations.

Steve Kellock, John Raymont and later Paul Lamb were most closely concerned with the payload operations. As the project progressed, and there was a period over which the launch date went back at the same speed as time advanced, we began to learn, together with the NASA Spacelab team, what participation in a Spacelab mission was to mean. Since it was a manned mission with real-time experimenter interaction, a high degree of skill was required of the experimenters in the operation of their instruments. Experience had to be gained in interacting with the instrument and with those controlling the spacecraft. An extensive series of training sessions was arranged by NASA, and it soon became clear how essential it was for the experimenters to attend them all, however expensive it was to have them making repeated lengthy journeys to the USA. The communications system at the Payload Operations Control Centre was quite daunting, with a 42-channel voice loop and as many as six channels to be monitored at any one moment. With a mission duration of a nominal nine days - likely to be cut down to less in the event of unforeseen problems - and a dedicated pointing session for the instrument of only 20 or 30 minutes at a time, any action by an experimenter had to be right first time. Computers were used to simulate the status of each instrument, including possible faults, and they responded to commands that the experimenter issued from a terminal for 'uplinking' to the instrument.

A considerable change in the method of tracking and acquiring data from satellites was coinciding with the first Spacelab launch, not by design but by the force of circumstances. NASA was replacing its extensive tracking and data relay system by two very large geostationary communications spacecraft, TDRS-A and B. The Spacelab 1 operation was devised in the expectation that both satellites would be operational, but they were not. Only A had been launched, on a Space Shuttle flight, and there had been initial problems in acquiring the correct orbit, so the Spacelab 1 launch had to be held up for a further month. As there was no TDRS B there had inevitably to be a large reduction in the real-time interaction with Spacelab. Foreseeing some such emergency, NASA had provided a data storage facility on the Space Shuttle so that data that should have been relayed via B were held and then dumped via A. If for operational reasons a dump did not take place there would then be many months of delay before the recorded data were recovered, processed and made available.

Spacelab 1, launched on 28 November 1983, was a 10-day NASA/ESA mission carried aboard NASA's Space Shuttle Columbia. The experimenters controlled their instruments from the NASA Johnson Space Center. They found the control and monitoring task as straightforward as if the instrument had been in a vacuum tank on the ground. On two occasions when the command and data links were not operational they were able to ask the Space Shuttle payload specialist to send commands using the on-board computer. Our instrument functioned correctly throughout the flight and remained within a safe temperature range despite the Shuttle payload bay sometimes being pointed directly towards the Sun. What is more, all scientifically-useful

data were acquired in near real time, so that there had to be no long wait for scientific data. Eighteen dedicated target pointings were obtained, all of which returned useful data. A bonus was that the instrument was returned in perfect condition and was calibrated again, this time at the SERC's Daresbury Laboratory. It was then available for another flight, should that prove possible.

An unforeseen difficulty arose later when it was realised that housekeeping and tracking data essential to the interpretation of the scintillator outputs were being routed through a lengthy supply path. When after eighteen months they arrived, the tape format was not specified, and so further delay ensued. Then many of the data records were found to be corrupted. A complete replacement set eventually arrived almost three years after the 10-day flight.

SPACELAB 2 CORONAL HELIUM ABUNDANCE SPACELAB EXPERIMENT

The ultraviolet spectrometer carried on the last Skylark rocket SL 1305 at Woomera was built jointly with the ARD Culham. It was to measure the relative abundance of helium to hydrogen in the solar corona, a parameter vital to theories of the origin of the Universe. Before completion of the instrument for the rocket flight, the possibility arose of flying a similar instrument, suitably adapted, in NASA's Spacelab 2. The proposal was accepted in 1977. However, as the required measurement accuracy of better than 10 per cent was not achieved during the rocket flight, and as the Spacelab requirements moved ever further from the rash 'ship-and-shoot' claim, a new instrument, the Coronal Helium Abundance Spacelab Experiment (CHASE) was devised and built, again jointly by the ARD and ourselves. The declared accuracy aim was initially 5 per cent though this seems later to have become 10 per cent again. CHASE was assigned to the sun-pointing platform to be carried in the Shuttle cargo bay, the launch date initially to be in mid-1982. As with Spacelab 1 the launch date slipped backwards steadily.

The MSSL task, which was managed by Norman, was to supply the ultraviolet detectors and associated electronics, the power electronics, a dedicated experiment microprocessor and its software, largely the responsibility of Paul Borrill, and the ground-support equipment. The latter equipment was used throughout the integration and testing of the instrument. When integration with the Shuttle had taken place, it was used to record data and to display engineering parameters.

Integration took place at the Kennedy Space Center. As with Spacelab 1, there were mission training sessions and mission simulations at the Marshall Space Flight Center and at Kennedy before the centre of operations and therefore the GSE were moved to the Johnson Space Flight Center at Houston. The mission operations required a 6-man team for each of the two 12-hour shifts in order to provide continuous coverage.

The Space Shuttle Challenger carried Spacelab 2 on its 8-day mission which began on 29 July 1985. CHASE worked exceptionally well throughout, despite the lower-than-planned orbit, which itself resulted from a premature engine shutdown. It probably yielded more useful data than any other of the solar instruments on that mission. Good thermal design by the MSSL mechanical engineering group, particularly good as they were forced to use a sunward-pointing radiator plate, ensured that CHASE did not overheat, unlike the other instruments on board. The dedicated experiment microprocessor was extremely reliable and was one of the few not to experience a system crash during the mission. Early in the mission the Shuttle crew were unable to lock the Instrument Pointing System on the Sun with its own sensor, but were able to when the CHASE sun sensor was substituted. Thereafter it was frequently used to control the system for solar observations, not to the same accuracy but sufficient (+/- 10 arcsec) for our needs.

NASA had intended to re-fly Spacelab 2 in September 1987 with, it was hoped, the CHASE instrument, but the loss of Challenger made that impossible.

ACTIVE MAGNETOSPHERIC PARTICLE TRACER EXPERIMENT (AMPTE)

An understanding of the complexities of the Earth's magnetosphere, with its huge scale, its dependence on solar activity and its rapid, minute-by-minute changes, calls for ingenuity and boldness in the design of new experiments. AMPTE was an example of a bold endeavour which went unrewarded in its main purpose, but which, for the ingenuity which we put into it, returned us five months of invaluable scientific data.

The AMPTE mission, a 3-satellite project carried out jointly by the USA, Germany and the UK, was originally to be a 2-satellite mission aimed at the release from a German spacecraft of “tracer” ions of barium and lithium into the solar wind just outside the magnetosphere and their subsequent detection by a US spacecraft in low Earth orbit. In this way it was hoped to gain much-needed information on how the energetic ions and electrons from the solar wind “leak” into the magnetosphere.

The contribution of a British sub-satellite, known as UKS, in the construction of which the MSSL played a major role, came about initially in 1980 through a personal contact between Alan Johnstone at MSSL, Duncan Bryant of the RAL magnetospheric group, and the Germans who were planning the mission. A Delta launch had already been arranged by the German and US groups with NASA, so providing ample payload capacity. The release arrangements, enabling the US satellite to be put into a low orbit and the German satellite into a highly eccentric orbit with an apogee of 20 earth radii (R_E), were such that the two spacecraft required the use of a sizeable adaptor or spacer between them. The Germans suggested that the British might be interested in providing the spacer in the form of a fully independent sub-satellite which would thereafter contribute to the US/German mission. Fortunately the ASR Board were persuaded that it was not only a worthwhile endeavour but one that could be accommodated in-house without financial difficulty, with MSSL acting as a subcontractor to RAL. It was not a decision that endeared itself to the one or two firms in the UK struggling to earn their daily bread in the spacecraft construction business, but for the ASR Board it was to be either an in-house project or no project at all.

For the MSSL the decision was a welcome one, particularly on the mechanical engineering side where work was becoming difficult to find. Furthermore Firewheel, an earlier, less ambitious project, had ended in the Gulf of Mexico, and the engineers were keen to try again. The decision was also one more example of SERC's changing attitude towards groups like the MSSL which it had funded heretofore so strictly. At last it was permissible for University groups to undertake extra projects with separate funding beyond the main grant and not be penalised for so doing.

Both UK groups were to provide experimental equipment. Coates took charge of the provision of an MSSL instrument measuring the 3-D distribution function of positive ions between 10 eV and 2 keV, using a development of the GEOS instrument. Johnstone had by this time forged an excellent working relationship with the group at the SouthWest Research Institute at San Antonio, Texas, and Coates and Steve Kellock were able to take the instrument there for detailed calibration.

Patrick and Sheather and their mechanical engineering team were to build the structure, the magnetometer booms, the active thermal-control system, the attitude and orbit control system

and the attitude sensing system. Jim Bowles and Phil Guttridge designed and built the electronics equipment, including that for the ion sensors, and the attitude control and sensing system. They might have done more had they not been so heavily committed on other projects, and it was sad for us to have to decline other electronics tasks. RAL were to provide the power supplies, the telemetry system and other common facilities.

The operations plan was for UKS to be stationed near the German spacecraft, the Ion Release Module, so as to detect the outward movement of each chemical release and to measure the Earth's natural environment in the absence of artificial releases. The triple launch on 16 August 1984 went well. The 77kg UKS satellite and the German satellite were put into a 17.8 Re orbit, near the 1200 local time position with apogee well out into the solar wind. Excellent measurements were made, with each of the seven ion releases being observed. Unfortunately the large-scale tracer experiment, around which the US/German initiative centred, revealed nothing, no barium or lithium ions being detected by the low altitude US IRM. The measurements by the German and British spacecraft were extremely successful, however, and have had much to tell us about the interaction of an 'artificial' injected plasma with the flowing solar-wind plasma. In addition, the measurements of natural plasma were made with unprecedented resolution.

Sadly UKS went off the air on 16 January 1985, after an all-too-short operational life, probably because of a failed telemetry transmitter. RAL had been constrained by their budget to adopt a cheap solution to an otherwise expensive choice of transmitter, and the project paid the price.

In the nearby University of Surrey a small group of satellite enthusiasts were creating a widely-publicised reputation for their ability to build small satellites for scientific and amateur radio purposes. Our UKS achievement at MSSL went almost unnoticed, yet it deserved as much or more public appreciation. We were perhaps too busy with a multitude of other activities to blow our trumpets over AMPTE. Looking back, however, I feel that all concerned must take great pleasure at such an achievement.

THREE MORE SATELLITES

ROSAT

Following an initiative by Ken Pounds' group at Leicester in 1981, the MSSL joined a consortium of UK experimenters to provide an extreme ultraviolet (60-400 Å) instrument known as the Wide Field Camera (WFC), to be carried on the German ROSAT (Röntgen satellite) spacecraft and launched on a Space Shuttle, the planned date being 1987.

The principle ROSAT instrument was a high-resolution imaging soft-X-ray telescope. The chosen wavelength range for the British instrument was one largely unexplored; our measurements were therefore designed to complement the main shorter-wavelength German telescope. The WFC team was led by the University of Leicester, the other members being the University of Birmingham, the Imperial College of Science and Technology, the SERC's Rutherford-Appleton Laboratory and the MSSL. The aim was to carry out a six-month all-sky survey, followed by about one year of pointed observations.

The MSSL's task, which was managed by Richard Cole, was to supply the command and data-handling system for the instrument, the power-regulation system, the electrical harness and the electronic ground-support equipment. It became a major demand on the resources of the electronics engineering group, the work of which was supervised by Phil Guttridge. The MSSL had also to supply the computer system for a quick-look data-analysis facility to be set up at the German control centre, at the Max Planck Institute, Garching. We were initially to have been responsible for the wide-field camera's tape recorder, but, following a study carried out by Dornier System, the spacecraft prime contractor at Friedrichshafen, it was decided that the tape recorders should reside in the spacecraft and become a Dornier responsibility.

I have thought it worth giving below a detailed record of the WFC/ROSAT programme as an example of the complexity of the work now required for a major satellite project. The engineering model of the wide-field camera was integrated at MSSL early in 1985 and delivered to Dornier System, with the ground equipment, in June. A second set of ground-support system, built around a PDP 11/44, had to be quickly duplicated so that flight-system work could overlap with the tests going on at Dornier, and for this Leicester lent us another PDP 11/44. There was also a need for a structural and thermal mass model, with considerable contributions from us. This too was integrated at MSSL and delivered in August 1985.

A WFC data-handling group was set up to provide for the needs of a UK data-handling centre at the RAL, for a UK Rosat survey centre at Leicester and for the quick-look facility at Garching, the latter being at first based on a VAX 11/730 computer, which it was our job to define and order. Later the choice was changed to a Micro-VAX II, with delivery not required until the end of 1988. Our main contribution as part of the data-analysis consortium was initially to prepare the command and checking software. It was agreed that MSSL would receive 23 per cent of the data from the initial six-month survey.

The flight model of the camera was integrated at MSSL. End-to-end tests first took place in September 1986 and were completed at the end of that year. Meanwhile preparations were made at Dornier for a five-day simulation of orbital operations, and this took place in January 1987. In May 1987 the flight model was delivered. Following tests, some modifications were required to

our units, and these were made and the units re-delivered. The thermal vacuum and vibration tests were carried out at INTESPACE at Toulouse, with our staff in support.

With the camera complete and tested, but without its flight mirrors, integration with the flight spacecraft began in Germany. This was followed by vibration, acoustic, thermal-vacuum and electro-magnetic compatibility tests. With these completed the instrument was removed from the spacecraft early in 1989 and returned to this country for the installation of the flight mirrors and detectors. More vibration tests were then required before a full end-to-end X-ray calibration at the PANTER facility at Munich. By July 1989 the flight camera was delivered to Dornier, for the last time, for final tests before delivery to Cape Canaveral. At about the same time delivery of the Micro-Vax-II was taken at MSSL for preparation and transfer to Garching later in the year. The software system for the processing and analysis of the data, provided by the data-analysis consortium, was delivered to Holmbury and installed early in 1990.

Because of the Challenger disaster, large delays in the launch schedule of ROSAT were inevitable, and eventually negotiations were completed by the Germans with NASA for the purchase of a Delta II launch. It took place on 1 June 1990. Both the satellite and its instrumental payload functioned extremely well. By January 1991 the mission was well advanced, with the wide field camera in its six-month all-sky survey. Following the survey there was to be about a year of pointed observations complementing the work of the main ROSAT soft X-ray telescope. Throughout, the MSSL has the task of providing mission scientist support at Garching and at the UK Data Centre at the RAL, of providing software support for the QLF, and of analysing its share of the survey data.

CHEMICAL RELEASE AND RADIATION EFFECTS SATELLITE (CRRES)

In 1982 the US Air Force began to plan a satellite project in which a series of chemical releases would be made from a spacecraft launched from a Space Shuttle into a low Earth orbit. The spacecraft would then be moved to a geostationary transfer orbit, going out to 7 Re, in order to map the plasma environment in the radiation belts over a period of two years or more. The spacecraft was to be built by the Ball Aerospace of Boulder, Colorado, and controlled operationally from the satellite control facility at Sunnyvale, California.

The MSSL was invited to join the Air Force Geophysics Laboratory, Boston, in providing a package of plasma instruments, with funds coming in part from the USAF. Our contribution was the Low Energy Plasma Analyser (LEPA), a developed version of the electron and ion sensors flown on AMPTE and Giotto giving an angular resolution of 1 degree over a 120-degree range. The exciting aspect of the project was that the satellite would be spinning slowly and the instrument would cover all pitch angles from 0 to 180 degrees once every 30 seconds. The completeness of the coverage and the continuity of sampling of the loss cones of both electrons and ions should then enable substantial progress to be made in understanding wave-particle interactions. The loss cones gradually fill as the particles cross the equator, so by sampling both loss cones it should be possible to understand the generation of natural or stimulated VLF chorus emission near the geomagnetic equator. The difference between fluxes in the two loss cones should also make it possible to identify where the satellite is in the diffusion region. The AFGL package would contain a full complement of wave-detection instruments. It was later arranged that the University of Sussex would provide a wave/particle correlator.

The prototype LEPA instrument was built and tested in 1984. In 1986 the flight model was integrated into the spacecraft but the loss of Challenger led to a considerable slip in launch date. The instruments were removed from the spacecraft and returned to the experimenters. During subsequent testing at AFGL high-voltage breakdown was found in our LEPA instrument at the

highest operating voltage, so the opportunity was available to have the fault diagnosed and corrected. With the Space Shuttle still causing delays a launch on an Atlas-Centaur was arranged, and re-integration into the spacecraft took place at Ball Aerospace at Boulder in June 1989.

The launch took place on 25 July 1990. Operations went very well and excellent data were received on the natural plasma. In early September 1990 the first chemical release was made, and more were to follow in 1991.

ASTRO-C

Work began jointly in 1981/82 with Pounds' X-ray astronomy group at the University of Leicester on the design of a large-area X-ray detector for the Japanese Astro-C mission. A collaboration was arranged with a number of Japanese groups, led by the Institute of Space and Astronautical Science, Tokyo, for the production and flight of eight large-area proportional counters, giving a total area of about 0.5 square metres, to form part of the main X-ray instrument on the spacecraft.

Having joined the project we found ourselves financially over-committed and were disappointed to find that the ASR Board of the SERC was not willing to enhance our annual grant. Forced to cut our suit to match our cloth, we had to drop something, and the choice fell on Astro-C. We had already participated in the production of the engineering-model counters, and had undertaken the design and manufacture of the 16 printed-circuit boards for the front-end analogue electronics. We had also designed and put out to manufacture an electrode-frame assembly. Having met our commitments for the prototype models we withdrew in 1984, handing over our part of the project to a group at the RAL. Withdrawal from a project was a new experience for us, so it was later exasperating to see the RAL making a successful application to the SERC for funds with which to complete what had been our project, but for which money could not be found!

REMOTE SENSING AND EARTH RESOURCES

Space science in the United Kingdom was, until the 1980s, concerned with the upper atmosphere, the ionosphere and astronomy. At the beginning of this decade, however, a growing, world-wide interest in climatic variability, fostered by the World Climate Research Programme, led the National Environmental Research Council (NERC), and SERC to respond with a UK activity, 'Joint Action for Climate Research'. It included encouragement to university groups not previously involved to participate in climate studies. Boyd, through his close connection with the SERC, being Chairman of the Astronomy, Space and Radio Board, had put his weight behind this initiative. There was a degree of reservation within the SERC about how far it wished to be drawn in - evidently the feeling in some quarters was that it shouldn't have to put its money where it's mouth was. However, with the door on the latch if not open, Boyd was determined that the MSSL should enter this new field. He knew it to be of great importance in its own right, but he was also aware of a decline in the support for space astronomy and he could see very well that the laboratory would benefit by having another string to its bow. He also knew that any effort by the MSSL should be well led and seen to be so. He therefore persuaded Chris Rapley to leave the SMM project (Chapter 14) and to head the remote-sensing activity. He went further and arranged with the College for a special post to be made available to Rapley by way of the 'New blood' scheme. In this way the MSSL extended its established skills in support of two specific research areas of importance to climate studies, the global measurement of sea-surface temperature and studies of ice and land by radar altimetry.

The first move into this new research field had been taken in 1981 jointly by the RAL and the University of Oxford. Houghton at the Clarendon Laboratory, Oxford, had been at the forefront of atmospheric research for many years. He and S.D. Smith at the Heriot-Watt University had developed a technique for measuring the temperature profile of the atmosphere below a satellite, and this had been employed with success on a succession of US Nimbus spacecraft, and on a Venus orbiter. They had recently extended the technique to the measurement of cloud-top temperatures. The project had been very much aided initially by Maddock's staff at the AWRE and later by a team at the RAL. It was therefore a logical move for the RAL/Oxford team to take an active interest in the new remote-sensing initiative, particularly as Houghton had become joint director of the newly merged Rutherford and Appleton Laboratories. The MSSL was later to join that partnership, bringing its extensive experience in the design and construction of space instrumentation.

By mid-1983 the partnership was launched on the construction of the along-track scanning radiometer (ATSR) for the first European remote-sensing satellite ERS 1, and the measurement of sea-surface temperatures on a global, synoptic basis. Rapley's growing research team was also at work on a variety of radar-altimetry investigations. Sea ice, lake levels, and even the land itself came to be studied using microwaves at normal incidence.

As the ATSR work progressed, preparations were put in hand for the MSSL's participation in the data-analysis phase of the radar-altimeter project on ERS.1. This preparatory work was based on data from NASA's Seasat mission, from an RAL airborne altimeter and from software simulators.

On 16 March 1990 Professor Sir Robert Boyd opened the Boyd Remote Sensing Centre. The buildings, a generous gift by GEC-Marconi, were brought to Holmbury in the summer of 1989

and erected on the Gladstone Walk site. They were then fitted out with accommodation for 18 staff and a range of computing equipment.

THE LARGE SPACE TELESCOPE

The Hubble Telescope, originally known as the Large Space Telescope, might appear to have no place in these reminiscences, but as it involved Sir Harrie Massey, Bob Wilson and myself for a while in 1975-76 it deserves some mention.

The design and construction of the new telescope in NASA had been in progress for many years when, in 1975, the US Congress suggested that in view of the mounting cost other nations, specifically ESA and European national space programmes, should be asked to contribute to its funding. In response, NASA began negotiations with ESA on a government-to-government basis. This had resulted in concern among scientists on both sides of the Atlantic who felt that there should be some coordinated advice from them to the several interested governments and agencies. Different management traditions, different approaches to instrument procurement, meant that, though each side understood its own system, each felt less comfortable with the other when setting about ensuring the best possible performance of the LST. The two sides were unequally prepared for discussions since the question of European involvement was of such recent origin. With the aim of establishing a foundation of basic ideas upon which the scientists could build, discussions had begun between the Space Science Committee of the European Science Foundation and the Space Science Board of the US National Academy of Sciences-National Research Council. The chairman of the space science committee was Sir Harrie Massey. The chairman of the Space Science Board was Prof. Richard Goody from Harvard University, formally a member of staff of the Department of Meteorology at Imperial College, a one-time colleague of P.A. Sheppard and an early member of his team that put forward proposals for the first Skylark experiments. The two chairmen, Massey and Goody, suggested that a discussion meeting should be arranged between interested scientists on both sides to establish these all-important foundations.

The US Space Science Board had come into existence when it was felt in United States there was a need for consultation between NASA and an independent body drawn from the scientific community which it served. There had been earlier difficulties and misunderstandings, principally because of the feeling that NASA scientists had made decisions about the future shape of space science without proper consultation. A formal system of consultation by NASA of the SSB had therefore been set up and it was working well. The SSB had long been involved in considerations pertaining to the LST. As early as 1969 its ad hoc 'Committee on the LST' had published a report on the likely scientific functions of the LST, and on the practical feasibility of placing it in earth orbit - or for that matter in moon orbit. At that time it was assumed that the telescope would be a diffraction-limited 120-in device. In the event the design aim was less ambitious, a little less than diffraction limited, with an 86-in objective.

Remembering what had led up to the NASA/SSB consultations, the US scientists had no difficulty in understanding the concerns of the European space-science community that they too should have some independent body, based on the SSC, which could make its representations to ESA, and through it to NASA also on the LST matter. They were therefore glad to cooperate in setting up the proposed discussion meeting.

Massey and I visited the National Academy of Sciences in Washington in November 1975 to meet Goody and Margaret Burbidge from La Jolla to lay plans for the discussion meeting. Margaret Burbidge learnt her physics and astronomy at UCL and so was as well know

personally to Massey, as she was in general to the whole UK astronomical community. She had, for a brief while in the 1970s, been Director of the Royal Greenwich Observatory. Our host was Milton Rosen of Viking and Vanguard fame (Chapter 1), now secretary of the Space Science Board. Having settled us in to our meeting he discreetly withdrew, saying that he thought it best if he left the British to themselves!

My task, following that meeting, was to gather together a representative group of astronomers from the various European countries and arrange for them to attend a meeting in January 1976 at the Williamsburg Motor Inn, a well organised motel-cum-conference centre at the historic town of Williamsburg, Virginia. There were nine astronomers, Bertola (Italy), Courtes (France), Golay (Switzerland), Grewing (Germany), van de Hulst (Netherlands), Graham Smith (UK), Wilson (UK), Wolltjer (ESO) and Wyller (Sweden), plus Massey and myself. Graham Smith was then Director, RGO. On the US side there were Margaret Burbidge (California), Danielson (Princeton), Field (Harvard), Findlay (NRAO), Friedman (NRL), Meyer (Chicago) and Neugebauer (Cal Tech), plus Goody. There was also MacRae from the David Dunlap Observatory, Ontario and thirteen observers, including six from NASA, Hinnens, O'Dell, Rasool, Nancy Roman, Spencer and Adrienne Timothy, and three from ESA, Machedo, Mellors and Peytremann.

The meeting was generally thought to have been a great success. Initially there was much to be learned, as few on the European side had more than the sketchiest idea of the project, and the whole of the first day was taken up by a series of talks on the telescope and the instruments proposed for it. The contributions by the project scientist and astronomer C.R. O'Dell from the Marshall Space Flight Center were particularly valuable. His youthful ambitions in astronomy had, he complained, been laid aside in the service of the LST, in which he had grown old, perhaps too old ever to return. And that was in 1976; the Hubble telescope was launched in 1990!

It was O'Dell who, in effect, asked the scientists to take the optical design of the telescope on trust. We were presented with as much detail as any of us could wish on the expected aberrations of the system. It was understood that the optical manufacturers were very experienced in the figuring of large reflectors. It was also tacitly assumed that they were making telescopes for spy satellites, hence their experience. No further information was volunteered or asked for.

On the second day rumbles of discontent came from the US side. George Field was their spokesman on a matter which, for an hour or more, seemed to be developing into a serious impediment to any further progress. The problem lay in the NASA/ESA agreement, so far only informal, that the ESA payoff for contribution to the LST project should be the right to contribute one instrument. Field pointed out that NASA practice was to call for instrument proposals and to submit them to peer review. In that way the best proposals were selected. It seemed to many on his side that ESA would be buying its way in with a proposal which could, in principle, be inferior to a US one. It was a difficult matter for Massey to deal with as chairman; it was an embarrassing matter for Field to have to raise. The coffee break proved effective for it allowed time for reflection and consideration, particularly consideration of the fact that without an assured position on the spacecraft there could be no ESA participation. That in turn could, if a certain US Congressman had his way, mean no LST project. When the meeting resumed Field very diplomatically announced that the US side would make no further reference to their objections. The matter was dropped. From then on the discussions proceeded in the best possible spirit.

After the third day it was clear that ESA's participation could proceed on the basis of the earlier inter-governmental agreement, and word was sent to NASA headquarters where Roy Gibson,

Director-General ESA, was on a visit. He and the NASA Administrator Jim Fletcher flew down to Williamsburg next day to participate in the discussions, to hear the conclusions and recommendations of the conference, and to set their seal of approval on them.

There were five conclusions and recommendations:

- The first was, in essence, the all-important statement of support for the LST project, as representing a unique improvement in astronomical technique.
- The second was a recommendation that, to ensure a selection of instruments that would fully exploit the telescope's capabilities, a joint NAS/ESF advisory committee should review all proposed instrument designs before selection. This was a valuable recommendation for the US side because it actually made it possible for a joint committee to reject the ESA proposal. It is of interest that at the commencement of the meeting there was still an infrared instrument on the short list, even though it could not have exploited the telescope's capability fully. Its proponent, Neugebauer, announced its withdrawal at an early stage.
- The third recommendation was that a scientific organisation, possibly a science institute, would be needed, and that it should have international participation at all levels. Its task would be to help guest investigators use the LST instruments, and to carry out astronomical research. The NASA representatives had made it clear that there was no wish at headquarters to be saddled with the inevitably large and continuing cost of an institute, if only because they would thereby create a precedent which might lead to rash of demands for such institutes. ESA took much the same view; it certainly resisted the idea of setting up a mini-institute somewhere in Europe, though it wished of course to play a part in any US institute.
- The fourth recommendation concerned the allocation of observing time. It echoed the earlier anxieties about ESA's special position by making scientific merit the prime consideration in the allocation of observing time. It then sensibly recognised that other considerations might affect allocations to a country, other than the United States, that had made material contributions to the project. That, I suppose, is known as having your cake and eating it!
- The fifth recommendation was a little less clear than its predecessors. It concerned the expected huge flow of data, which could take the astronomical community unprepared, and called for a study to be made of the problem of handling it. The science institute would at that stage have handed on the data. The recommendation was therefore posing the question, 'What steps will you in the UK, or France, or Germany, or wherever, be taking to ensure that when you do receive your observational data you are properly prepared to take care of it?'

The report of the meeting, later published jointly by the NAS and the ESF, was written by Margaret Burbidge, Bob Wilson and myself on our last day at Williamsburg, again, rather fittingly, an all-British group!

Having received the go-ahead for its participation in the LST project, and having agreed its contribution to the project hardware, principally in the provision of the solar cells, ESA commenced its instrument selection procedure. The choice fell on Boksenberg's Image Photon Counting System. It was taken over by ESA and re-engineered for space operations.

Despite its initial reluctance to contemplate setting up and funding a science institute, NASA quickly came round to the realisation that without such an institute the potential of the LST would not be realised. It therefore asked the Space Science Board of the NAS to undertake 'a study of possible institutional arrangements for the scientific use of the ST'. Notice the change

of nomenclature from LST to ST, because the mirror diameter had been so severely reduced during the early 1970s that by 1976 it was considered to be no longer “large”.

The study group was made up of twelve US astronomers and four scientists chosen for their experience of running research laboratories (Kitt Peak, NRAO, National Accelerator Laboratory and NCAR). The chairman was Dr Donald Hornig, one time scientific adviser to President Lyndon Johnson, recently president of Brown College, Rhode Island. The group met in Washington for three days, including one at the Goddard Space Flight Center for a briefing on the ST. Subsequently it met for ten days at Woods Hole, Massachusetts. I was invited by NASA to attend as an ESA-nominated observer, together with Woltjer of the European Southern Observatory in Chile, who arrived only at the end. Others there who were at Williamsburg were Goody, Field and Rosen.

At the heart of the discussions were the questions:

- was an institute needed at all?
- if so, what arguments are most likely to weigh sufficiently heavily to convince Congress?
- what functions should the institute perform?
- what form should the institute take?
- and, a topic that provoked more discussion than any other, where should the institute be sited, in or out of the Goddard Space Flight Center, and if out, where?

The study group participants represented every major astronomy interest in the USA. This was no longer a joint USA/Europe body but a purely internal US group of astronomers who were not, by and large, well experienced in space matters. They were, however, well experienced in the day-to-day dog fight between those with conflicting interests, and their rivalries were well aired. The European interest which I was there to represent got short shrift, understandably as ESA was at that stage not prepared to be committed to anything.

From one quarter there came very strong anti-Europe feelings. Why, it was asked, was Europe invited to sit at this table when, for example, Japan wasn't? Why an obvious bias, when discussing the location of an institute, towards an east-coast site and ease of access to Europe? Wasn't ease of access to the West as important as to the East? Let there be no special arrangements to suit Europeans. The most strongly voiced objections came from an ex-patriot Englishman who, now a naturalised American, could not conceal his bitterness towards the UK.

After five days of long and intensive discussions, sometimes in sub-groups, the report writing got under way. Interestingly, my status as an ESA observer in no way barred me from being recruited to a full part in the task, and my written contribution, whatever the topic, weighed equally with any other. After ten days, with most of the participants having already departed, I helped see the job to completion. A lengthy report had been written, detailing the structure and staffing of the Institute, the functions it was to perform, the duties of the staff, the facilities with which they were to be provided. There were twenty-seven recommendations, one only reflecting an international interest - ‘We recommend that arrangements be made for international participation in the Institute, including its policy-making bodies.’ As to the location of the Institute, there was no compelling reason found that would dictate location at any specific site, but the report was quite clear that the scientific productivity of the Institute would be enhanced if it were located elsewhere than within GSFC.

Eventually the Space Telescope Science Institute was set up at Johns Hopkins University, an excellent choice in every way. It is run by AURA, under contract with NASA, one of the two corporate bodies in the USA with experience in managing universities. Its first director is Ricardo Giacconi, an X-ray astronomer. One of our former students, Adrienne Timothy (before she remarried), has (1991) a senior position there.

EPILOGUE

When in 1976 we drew close to our tenth anniversary at Holmbury Robert Boyd suggested that we should mark it with an exhibition, not at Holmbury where we would be hiding our lights but at College where everyone would have a chance to find out about our work. Having prepared ourselves fully we moved the material to Gower Street one Saturday and set up an absolutely stunning display in the cloisters over the weekend. College staff and students arriving on the Monday morning found the place transformed. We invited many guests, and, once again, the Press. We were warmly congratulated from all sides, but I recall with the greatest pleasure the generous tribute paid to us by a professor from the Arts faculty. ‘I have very little idea what it’s all about,’ he said, ‘but what pleases me is how beautiful all your equipment is.’ Of course he was absolutely right, and I went round the exhibition once again to look with a fresh eye. When scientific instruments are well-designed, well made and well-presented they have an unconscious beauty. When, allied to that, there are the additional requirements of lightness and finish, perhaps in rhodium, chromium, silver or even gold, the result can be almost extravagantly good looking.

The possibility of early retirement had for some time been in my mind when in 1982 financial pressures on the universities led to a scheme that would make it possible for permanent staff to go at 60 rather than at the standard age of 65. After 17 years of administration I was ready for a change. I had begun teaching an astronomy course several years before and that had been very stimulating, but it wasn’t enough. So when Tovell reminded me of my earlier interest in retirement with the news that a scheme was now available, I seized the opportunity. Wing Commander ‘Bill’ Angell, who had been my assistant for many years—as excellent a colleague as I could have wished for—was shortly to retire, and Robert Boyd too was thinking of going, so it was a time of change. My retirement would have one serious disadvantage, I would leave no vacancy to be filled—only Angell’s post would be vacant.

Boyd retired at the end of September 1983. Len Culhane, for many years his deputy, succeeded him as Director. Andrew Thomson started work at about the same time. A personal tragedy marred that year and left me with only blurred memories of those changes—my wife Clare died at the end of June 1983 after a year-long illness. I retired at the end of September 1984; but under the retirement arrangements I continued to teach for one more year.

My personal knowledge of the events covered by these reminiscences would therefore appear to have ended in 1984/85, shortly before the laboratory celebrated its 20th anniversary. But I have stayed in touch with events through the very welcome task of editing the MSSL annual report. At each visit I have taken much pleasure in finding the MSSL larger than last time, better cared for, better equipped, better organised,—in one word, flourishing. There was never any guarantee that this would be so. Holmbury did not come into being as a UCL initiative but as a departmental venture, and as such it could so easily have shrunk and vanished under the repeated stresses of financial crises. That it has not done so speaks volumes for the good sense and loyal support of the College, the Department and of course the SERC. It speaks more than volumes for the staff of MSSL who over the years have given this university space science laboratory a reputation second to none in Europe.

Eric Dorling March 1991