Research begins

Opportunities

For those who were ready to imbibe Copland’s pioneering spirit, the University in the 1950s offered opportunities in abundance. There was money, vastly more than academics in the state universities were used to; and although funds were reduced during the decade by high inflation and limits on government spending, there were still generous budgets for equipment, travel and new appointments to the academic and support staff. There was time, a luxury for most academics burdened with heavy teaching loads, but especially for those whose research activities had virtually ceased during the war. Freed from teaching responsibilities (except for the occasional PhD student), the professors, as Sawer remarked, lectured to one another. And there was the richest opportunity of all, the freedom to range across the vast tracts of uncharted intellectual territory and select a field of research for intensive cultivation. Sawer wrote what many others thought: ‘Bliss was it in that dawn to be a Don’.

The planners had assumed that the new University should have research facilities as good as or better than any other research institution in the world. This implied that the staff should have ample opportunities for overseas travel, to keep abreast of developments in other countries and to establish the University’s place in the international scholarly community. Professors were therefore given a ‘sabbatical’ leave entitlement of one year in four, significantly better than the usual one year in seven, along with generous travel allowances.

The first staff were encouraged to inspect relevant research centres in other countries. On his way to Australia, Crocker, at Copland’s request, toured universities in the United States, Britain and Europe, and prepared a lengthy report on research in international relations. Wes Whitten, the Animal Veterinary Officer, was sent around the world to inspect animal houses and then design one suitable for Australian conditions. FitzGerald, on his way to Australia and with £3000 to spend, went shopping for books in Hong Kong and the United States, and assembled the basis of a library of some 25,000 volumes, mostly in Chinese and including many classical works which would soon become difficult to obtain. The Librarian, McDonald, spent several months in 1950 visiting libraries and booksellers in Britain and the United States.

McDonald had been appointed early in 1948, before any of the professors. This reflected the priority that the Interim Council gave to the Library. Initially he and several assistants were housed in Ormond College in the University of Melbourne, where they brought together some 40,000 volumes. At the end of 1950 these were transferred to Canberra where, despite cramped conditions in the old hospital building and nearby

The first University Librarian, A.L.G. McDonald, 1953.
huts, the collection grew to 110,000 (excluding pamphlets) by the end of 1935 and 150,000 by the time of McDonald’s retirement in 1960. McDonald fashioned the Library’s acquisitions policy to meet the specific needs of the research schools, avoiding less directed purchasing which might lead to overlap with the holdings of the National Library, whose permanent building was destined to occupy a site just a few kilometres away. With large annual budgets, he was able to give the schools the books and periodicals they wanted: he was an academics’ librarian, and he created a library well suited to their immediate needs. On the debit side, he wished upon the University the inaptly named and relatively untired Bliss system of classification, on the grounds that the widely used Dewey system was inadequate for science and the Library of Congress classification was too hard for the Library’s inexperienced staff.

What Florey had said about medical research was true, and it could be applied with equal force to nearly all the other natural and social sciences: Australia had contributed little to the world of learning, except through its sons who had decided to live abroad. At the 1948 conference on the Social Sciences school, senior academics had given a mostly gloomy view of the research achievement in their disciplines. In economics, said S.J. Butlin from Sydney, university research was ‘very narrowly limited in range and generally at a comparatively low level’. In constitutional law and economics, said G.W. Paton from Melbourne, there had been much scattered work on the fringes; but there was ‘vast scope’ for research, while the whole field of Australian legal history had been ‘shockingly ignored’. At the Pacific Studies conference the Sydney anthropologist Ian Hogbin described Melanesia as ‘virtually a terra incognita’, while one of his Sydney colleagues remarked that the study of linguistics in the Pacific was in its infancy: ‘to set out the problems that await research is therefore practically to set out all the possibilities of the field’. Oliphant’s chosen area of nuclear physics was entirely new to Australia.

Postgraduate training scarcely existed before the war and it was not until 1945 that the PhD degree was introduced at the University of Melbourne, with the other state universities soon following. In 1950 there were still only a dozen PhD graduates from all Australian universities. In later decades, aspiring PhD students in many areas would have to search for a topic and ascertain whether or not it had been ‘covered’ already. But in the 1950s they could begin their explorations confident that no researchers in Australia had trodden that path before, or that if they had their journeys were well known.

The professors were free to follow research of their own choosing. Some continued the work they had been doing before joining the ANU; others branched out in new directions. The degree of freedom extended to non-professorial staff and students differed from one school and department to the next, depending on the nature of research being conducted and the plans of the head of department. But whether academics were working on their own or as part of a team, there was a prevailing sense of optimism. David Curtis, a young Research Fellow in the Department of Physiology, reflected as an Emeritus Professor in the 1990s that he was ‘very privileged having gone through that period. Well, I’m still very privileged now, but that to me was the most exciting time of my life.’ There was so much waiting to be discovered.
The John Curtin School of Medical Research

In the John Curtin School, Florey remained a dominant presence, even from the distance of Oxford. He corresponded regularly with the professors, offering encouragement and advice, and castigating anyone who in his view stood in the way of scientific research. Every year or two he visited Canberra, giving himself enough time to dissect an echidna (whose unique properties as a monotreme invited investigation) and, verbally at least, to take his scalpel to an architect or administrator. During these visits, the lights in Hugh Ennor’s laboratory, visible from Florey’s top floor flat in University House, burned late into the night. Yet while he served as an inspiration to research, Florey did not attempt to influence the work of the individual departments, except in relation to his own field of experimental pathology.

The formal arrangement was that Florey was Academic Adviser to the school. Early on he had suggested that Ennor might become ‘Temporary Assistant Director for the whole school [without prejudice to later arrangements]’. Copland decided otherwise, and for its first few years the school remained without any head other than its Adviser. In 1953, after all the departments except Medical Chemistry had gathered in Canberra, Ennor was appointed Dean, a title then in use in the social science schools. As such, he was regarded as chairman of the professors and representative of the school for administrative purposes. His role was viewed, especially by his colleagues, as purely administrative. There could be no question of his exercising any form of intellectual leadership.

The research directions of each department were therefore set by the individual professors, which was what they wanted and what Florey had intended in his early plans for the school. Adrien Albert in particular was adamant that he should have free rein to run the department of Medical Chemistry as he thought best. As he insisted on remaining in London until the permanent building was ready for occupation, his...
department grew in isolation from the rest of the school. The move to Canberra eventually took place towards the end of 1956, when every piece of equipment, right down to the last beaker, was carefully packed and reassembled in the new laboratories.

Albert’s work, and therefore the work of his department, focused mainly on exploring the chemical structure and physical properties of certain organic compounds, especially a group called the pteridines which had significant potential for the development of new drugs. This led to a fascination with heterocyclic compounds, which play a vital role in the metabolism of living cells. The department accumulated a mass of data on these substances and Albert devised a new classification system to describe them. This was classical organic chemistry, often pioneering and always of high quality, yet having no direct or necessary relationship to the rest of the school’s activities. Given Albert’s request at the time of his appointment that the department should be called Medical Chemistry, this research orientation was surprising.

In many ways, Albert epitomised the dedicated scientific researcher: enthusiastic, meticulous and encyclopedic in his knowledge of the field. According to his long-term colleague, D.J. Brown, he operated on the simple principles that time was the most precious commodity in life and that, in scientific research, the work was infinitely more important than the worker. Lean and elongated, he spent long hours stooped over the bench, starting late in the morning (owing to a medical problem) but remaining there until midnight, often seven days a week. He demanded much of his staff and supervised his students closely, so closely in fact that some decided to pursue their careers elsewhere. But those who stayed with him comprised a cohesive and productive team.

The starting point for work in Biochemistry was Ennor’s fascination with compounds containing phosphorus, especially their role in producing muscular energy. This relatively new area of research promised a better understanding of animal and human metabolism. Results were often unexpected, as when Harry Rosenberg, then a research scholar in the department, stumbled on a new method of estimating the presence of arginine, one of the amino acids which form an essential part of the human diet. His discovery became the standard method used in biochemical laboratories throughout the world.

Ennor relished the role of pioneer. Rather than purchasing (at high cost) certain compounds they needed from overseas, he and his colleagues would ‘knock off a few rabbits’ and spend a couple of days extracting what they needed. The school’s own workshop built much of their equipment, such as a micromanipulator which, Ennor told Florey, had everything except the ashtray. On the other hand, facilities for meeting researchers’ personal needs were to be Spartan, as befitted pioneers: Ennor insisted that the Biochemistry floors in the permanent building should be joined only by a goods lift, so that he and his colleagues would have to use the stairs.

In the early years, Ennor spent many hours at the laboratory bench, where he was, according to Rosenberg, ‘full of vigour, excellent wit, and boundless energy’. He was a great encourager, taking an interest in everyone’s work, but leaving each researcher free to do as he wanted. Relations in the laboratory were relaxed, even familiar: but Ennor was always addressed as ‘Prof’, never as Hugh. When an American visitor remarked that this seemed odd, given that Ennor was not much older than some of
his colleagues, Rosenberg simply replied ‘This is the way’. Gradually Ennor moved away from the bench, devoting more of his time to administration at school and University level, to lunches with Cabinet ministers or senior public servants, or attending high commissions and embassies on their countries’ national days. He continued to read and encourage the work of his colleagues; but the initiative for research moved entirely to the non-professorial staff. In contrast to Albert, who was scrupulous in ensuring that research papers were accurately acknowledged, that did not stop him putting his name to papers which he played little part in generating, a practice familiar to scientific researchers. Rosenberg shrugged his shoulders: this too was the way.

Staff and students throughout the medical school often described their own work as ‘fundamental’ research, as distinct from ‘applied’ research, which they regarded as properly the province of the CSIRO. Rosenberg appreciated the relevance of the University’s motto. ‘In those days it was perfectly clear what we had to do’, he reflected many years later: ‘We had to ... find out the truth about nature’. Research was driven by curiosity: so long as they did good and inspired work, nothing else was asked of them. In Medical Chemistry, Albert frowned on applied research, even though much of the department’s work had obvious biological implications. The job of the medical chemist, in his view, was to discover and explore the nature of compounds, leaving others to show how they might be used.

In practice, fundamental research was rarely as refined as it was made out to be. Florey argued that his own experiments were undertaken for their intrinsic biological interest, without practical objectives. Yet Henry Harris, his student as an ANU Overseas Scholar, noticed that nothing he did was far removed from some major human disease, and indeed that almost all his work related in some way to illnesses from which he or close members of his family actually suffered. Few if any medical scientists embarked on a project without some expectation or hope of practical applications, sooner or later. This meant that the ultimate goals of fundamental and applied research were often the same, differing only in their distance from the research itself and the clarity with which the researcher perceived them.

Whatever the stated objectives of a research project, results were best expressed in terms of practical applications. In the departments of Medical Chemistry and Biochemistry, the raw data of achievement, numerous papers published in prestigious journals and many lectures presented to international conferences—was impressive to fellow scientists. But there was little point in announcing to people who
did not know the difference between a pteridine and a pterodactyl that the Department of Medical Chemistry had discovered, over the years, some hundreds of compounds. Although Florey had always warned against encouraging people outside the University to expect dramatic results, there was nevertheless an understanding that a research school which housed some of the nation’s best scientific brains and which was using so much of the taxpayer’s money should produce significant results, meaning results which contributed to the health of the people. Even Albert, looking back in later years on the work of his department, emphasised how discoveries in the broad field of medical chemistry had led to the design of new drugs, and how those drugs had contributed to advances in chemotherapy, and helped in the treatment of malaria, leukaemia, gout and so on, as well as facilitating organ transplants.

George Mackaness, who in Florey’s absence ran the Department of Experimental Pathology, acknowledged that science had to appeal to the wider community. Experimental pathology, he told Florey, was better able than other departments to show visitors to the school what a quarter of a million pounds of their money was doing for them. He therefore urged that the person appointed to the chair should be engaged on work with human interest which could easily be interpreted to politicians and the press. This was ‘a loathsome desideratum’: but Mackaness contended that Australians were quick to judge, if often lacking in their judgement; and while teaching was recognised as a legitimate occupation, research was not. Research therefore had to be justified in terms that ordinary Australians could understand.

Mackaness went further than most of his colleagues in suggesting, in effect, that public appeal should influence research directions. But many people in different parts of
the University well knew that the University had to communicate with the person in the street. After Copland, the consummate publicist, had left the University, Coombs thought more effort was needed in public relations, and suggested that the University approach members of parliament, leaders in commerce and industry, the press and trade unions. When the biochemists began to extract substances from crocodiles and, with the help of Trans-Australia Airlines, arranged a crocodile hunt in the Northern Territory, he saw the opportunity to draw a link between research and an adventure, which was much easier to communicate than what the biochemists were actually trying to do.

Frank Fenner’s work in the Department of Microbiology showed how fundamental and applied research merged. After taking up duty with the University in 1949, he initially continued the bacteriological work he had been pursuing at the Rockefeller Institute; but he was keen to return to virology and was, as he put it, looking for a virus to work on. He found one early in 1951. During a stopover on a flight from his laboratory in Melbourne to a meeting in Canberra, he and Hugh Ward, Professor of Bacteriology at the University of Sydney and a member of the ANU Council, were shading themselves under the wing of a DC3 at Albury Airport. Not far away, along the Murray valley, an outbreak of myxomatosis was decimating the rabbit population. ‘You ought to get into that’, said Ward. Fenner took the hint, wondering why he had not seen the opportunity for himself, and immediately threw himself into studying the myxoma virus. A ‘fortunate change has come over my circumstances’, he told Florey. Myxomatosis was to occupy much of his time over the next fifteen years.

Rabbits had provided the stimulus for the beginnings of microbiology in Australia in the 1880s, when the New South Wales government offered, unsuccessfully, a large prize for the discovery of a biological means of controlling the rabbit plague. Prompted by the Melbourne paediatrician Jean MacNamara, the CSIR initiated experiments with myxomatosis in the 1930s, and again between May and December 1950. CSIRO scientists were about to give up when the infection took hold with astonishing effect in the warmth of the summer, especially along the waterways in Victoria and New South Wales. The obvious conclusion was that the virus was carried by mosquitoes.

What intrigued Fenner most of all were the theoretical aspects of the disease. This was an extraordinary natural experiment: a highly virulent parasite among a completely susceptible host. How, he asked himself, would the virus evolve? What would happen to the host animal, given that the virus was killing nearly all of the rabbits it infected? As he later recalled, ‘it was a marvellous opportunity for somebody interested in the evolution of infectious diseases’. Paradoxically, perhaps, Fenner and his small team viewed the relevance of their research to the rabbit problem as a side benefit. They were well aware of the potential economic significance of their work and its value to the nation, not least because it ensured that they would always be well funded. And they were certainly keen to bring the rabbit problem under control. As the virulence of myxomatosis decreased, they were out in the field with CSIRO scientists, investigating ways of maintaining its strength. But from the virologist's perspective, the nature of the virus, and its implications for research into other viruses in animals and humans, were more deeply interesting.
In the event, science played little part in enhancing the success of the natural virus. As rabbits developed resistance, its influence steadily declined: though in the 1950s it still helped control the rabbit population. But the study of myxomatosis yielded rich results. As well as identifying the distinctive properties of the virus and its relationship to other viruses, Fenner and his colleagues were able to explain why the virulence of the disease declined rather than increased, as it usually did in laboratory experiments. And by monitoring the resistance of rabbits over several generations, they were able to show that this resistance had a genetic basis. This was a major discovery in virology, and it led Fenner to further explorations of animal virus genetics.

Experimental science rarely proceeds in a straight line, from one significant discovery to the next. Fenner, along with almost every other researcher in the John Curtin School, sometimes followed leads which turned out to be false, having then to abandon them and set out in other directions. Mackaness, after eighteen months in Canberra, told Florey that his research was progressing as it had done in Oxford: ‘Many failures and partial success, followed by an era of productivity’. One of the tasks of the researcher, he wrote, was to distinguish between perseverance and stubbornness, ‘for one grades imperceptibly into the other’. As his own research became ever more frustrating, Mackaness joined his colleague Leigh Dodson, whose research on the effects of pregnancy on hypertension (abnormally high blood pressure) was showing great promise. Through experiments on a large colony of hypertensive rats, Dodson showed that during the latter half of pregnancy their blood pressure returned to normal levels, sometimes quite abruptly. Then, within two hours of parturition, the blood pressure returned to its previous hypertensive levels. Dodson looked for an explanation: did foetal kidneys produce an anti-hypertensive agent? Or did it have something to do with hormonal balance during pregnancy?

After laborious research, Dodson and Mackaness stumbled unhappily on a more prosaic explanation: a laboratory technician, presumably inadequately supervised and quite unaware of the implications of what he was doing, had been adjusting the anaesthetic administered to produce normal blood pressure levels while the rat was
under examination. Many months of work had been wasted; and the result was acutely embarrassing, especially since Ennor had brought the Vice-Chancellor to view this exciting research, just as the truth was revealed. The outcome was an example of the sheer bad luck that sometimes attends scientific research; but it also suggested that research could be skewed by the eagerness for results, especially when expectations were high. For Mackaness and Dodson, the incident was a major setback, although both went on, in different spheres, to pursue distinguished scientific careers.

Research in the Department of Physiology built on the neurophysiological work that J.C. Eccles had been pursuing at the University of Otago. Neurophysiology was one of the newer medical sciences, having taken shape as a discipline during the previous few decades under the guidance of Eccles’ mentor at Oxford, the Nobel Prize winner Sir Charles Sherrington. The war gave it a hefty boost through the development of radar and other techniques which could be adapted to biological purposes. Now it was possible to use oscilloscopes (a rarity before the war) to provide visual images of electrical impulses from the brain, and cameras to record the images, frame by frame. In New Zealand Eccles had worked closely with a physicist and instrument designer, Jack Coombs, who transferred to the ANU in 1952, bringing with him across the Tasman the highly specialised equipment that he had designed and built in Dunedin. This included four electrical stimulating and recording units which were critical to Eccles’ research during his fourteen years in Canberra and which continued to be used until the mid-1980s.

Eccles and his colleagues set out to explore the nature of the nervous system, and specifically how electrical messages or nerve impulses act across zones of close contact (synapses) between nerve cells. Previously, the nervous system had been analysed purely in terms of its output, by measuring reflexes. With the aid of Coombs’s recording equipment, and by inserting into nerve cells extraordinarily fine glass tubes containing conducting solutions, Eccles was able to show how the cells related to one another. The breakthrough, achieved before the move to Canberra, was to distinguish the processes of excitation and inhibition of cells, and to demonstrate that inhibition was itself an active process in the function of the brain. That discovery meant investigation of the brain could be conducted on new levels.

Research on the nervous system demanded wide-ranging skills in physiology, pharmacology and electronics. Eccles sought the best people wherever he could find them, so that Physiology quickly became the most ‘international’ department in the University. In the first five years the team comprised, besides Eccles, three Americans, two Australians, two New Zealanders, two Swedes, and one each from Mexico, Canada and England. They worked in groups of three or four, a number dictated by the size of the laboratories.
Eccles was an inspiring and profoundly knowledgeable leader, who knew the literature of neurophysiology so well that he could quote relevant material by volume and page number. In his approach to experimental research, he had been influenced by the British philosopher, Karl Popper, whom he had come to know when Popper was in Christchurch. Popper advocated scientific enquiry through two stages of investigation: the development of a hypothesis using the creative imagination; and the attempt to falsify this hypothesis by challenging its most vulnerable aspects. Eccles subjected his own theories to this process of challenge, and urged his junior colleagues, if not to follow Popper to the letter, at least to pursue the most rigorous methods during experimental research.

The neurophysiologists did all their experiments on cats, which were convenient to work on, easy to obtain, and cheap, their only drawback being occasional protests from animal welfare groups or furious cat owners who charged that their pets had been stolen by the University. When an experiment was arranged to take place, a group would start work at 7.00 or 7.30 a.m. to prepare and anaesthetise the animal and prepare the equipment. By 5.00 or 6.00 p.m. everything was ready for the cat to be moved into one of the laboratories which were shielded against electrical interference. They would then work all night, going home briefly for a shower and breakfast, and returning to work on the cat throughout the next day and perhaps the day after that. Many hours were spent waiting for things to happen, thus allowing time for a nap or for keeping up with the literature in the nearby medical library.

'It was a time of intense discussion and a great interaction between people', David Curtis remembered in the 1990s. 'We used to work all the week. We were usually in Saturday morning till mid Saturday afternoon getting ready for experiments the following week. Sometimes you'd go in on Sunday nights to do things.' When a series of experiments was over, Eccles (if he had been involved in the project) usually wrote the first draft of a paper; then there was a seminar or two, often lively discussion, and preparation of a final draft which represented as close to agreement as the group was likely to come. This process led to a string of publications through the 1950s, some of which were of seminal significance. 'As I look back on those years', wrote Eccles in 1977, 'I can hardly imagine how we dared to attempt so much!'.
The Research School of Physical Sciences

Oliphant’s overwhelming ambition when he arrived in Canberra in August 1950 was to build a machine to split the atom, a machine which was more powerful, yet cheaper, than anything comparable anywhere else in the world, and which would thrust Australia unequivocally into the nuclear age. Having played a part in creating the atomic bomb, and having seen its horrifying effects, he had determined to have nothing more to do with the development of nuclear power for military purposes; but he was convinced that the peaceful uses of atomic energy offered new hope to humanity. Once the energy was harnessed, he declared, the world would enjoy ‘a period of unparalleled prosperity’, with man’s work ‘reduced to a vanishing point’. As yet, little was known about the laws which governed nuclear energy: but ‘We are passing through a similar period to that which preceded Sir Isaac Newton’s formulation of the laws of gravity’. The sort of science he wanted to do, he once told Wright, was ‘where we let our imaginations go and leave it to our wits to devise the experimental procedures’.

Oliphant’s mentor, Lord Rutherford, had been first to split an atom in 1919; and in 1932 two of Oliphant’s colleagues in the Cavendish Laboratory, John Cockcroft and Ernest Walton, built a particle accelerator which fired protons towards atoms at such high energies that the atoms disintegrated. Soon afterwards Oliphant himself had designed and built an accelerator which, though less powerful than the Cockcroft–Walton apparatus, produced many more particles and directed them in a fine beam towards their target, increasing the likelihood of the atoms being broken apart. The machine yielded a wealth of discoveries, including two new isotopes, and won Oliphant his Fellowship of the Royal Society. It also left him with a compelling question: what would happen if atoms were bombarded by particles from more powerful accelerators?

The move to Birmingham in 1937 gave him a chance to find out. Soon he was building a new type of accelerator called the cyclotron, modelled on one then nearing completion in the United States, and vastly more powerful than his machine at the Cavendish. It was to be the largest accelerator in Europe. But war intervened, Oliphant was diverted into other tasks, and not until 1950 did the cyclotron produce a beam of particles. In the meantime, Oliphant had started work at Birmingham on another new type of accelerator, the proton synchrotron. He had also accepted the job in Canberra, with the promise it held of building a still bigger and better machine in his own country.

Oliphant announced his plans for this new accelerator in Nature early in 1950, before he set out for Canberra. He called it the cyclo-synchrotron, as it combined elements of both the proton synchrotron and the cyclotron, and it was to produce a particle beam of one thousand million electron-volts (MeVs). The first version was a synchro-cyclotron producing 200 MeV protons, which were then to be injected into an air-cored magnet at the periphery of the synchro-cyclotron magnet to increase their energy fivefold. The source of power for the air-cored magnet was to be a homopolar generator, comprising a heavy metal disc spinning rapidly in the strong magnetic field of the synchro-cyclotron.
The Research School of Physical Sciences about the time Oliphant arrived to take up duty in 1950. Florey had warned him to expect ‘a hole in the ground and a lot of promises’.

magnet, and capable of delivering very large current pulses when stopped suddenly. He noted that the homopolar generator might also be useful for other applications.

Oliphant predicted that the cyclo-synchrotron would be working in two to three years. But by 1953 he and his team had come to realise that their design, as well as being exceedingly complex, would not represent the great advance they had hoped for on the Birmingham synchrotron and would not equal the output of accelerators now under construction in the United States. This was a major setback: Oliphant, however, turned it to advantage and decided to convert the magnet of the existing half-built accelerator into a larger homopolar generator as the power source for a new air-cored magnet of a proton synchrotron providing particle energy ten times greater than they had originally aimed for. With this enormous energy it would be possible (so theory suggested) to produce the anti-proton, the negative particle whose existence had been predicted in 1930.

There was a problem: for reasons of economy of design, Oliphant’s new accelerator would produce a particle beam only once every ten minutes, which was slow by current standards and much slower than the rate of other machines then being built. That was the price Australia had to pay if it was to have its own accelerator. And the prospect of building a successful accelerator for a fraction of the cost of its rivals became part of the challenge. The team pressed on, borne along to a large extent by Oliphant’s drive and enthusiasm. In his Bakerian Lecture, delivered to the Royal Society in 1955, he announced that the Canberra accelerator would probably be completed in ‘about three years’.

Oliphant, Hancock told Florey, was the most optimistic man he had ever met. There were, of course, doubters, those who said an Australian accelerator would never be built, or that if it was it would never achieve the predicted results. Equally, there
were many who were convinced that the development of nuclear energy for peaceful purposes represented the way ahead. 'No one knows where this may lead', wrote one journalist: 'Possibly in 10 years' time scientists may look back on our present atom theories, atom bombs, and atomic energy machines as we look back on the first crude radio sets and theories of wireless waves.' In parliament there was bipartisan support. W.C. Wentworth, Liberal backbencher, predicted that the accelerator would enable man to create matter out of energy. Labor leader H.V. Evatt spoke of Oliphant as one of the two foremost scientists in the world: 'and in Oliphant, we have an Australian'.

Oliphant's optimism was the optimism of the times. Wentworth, one of the best informed of his supporters in the non-scientific community, recognised there was a chance that the machine might not work. But the prospect of a world with nuclear energy in the service of humankind and Australia at the forefront of nuclear technology was so enticing that he and others were prepared, like Oliphant himself, to set the risks aside. When Oliphant's spirits flagged, as they did from time to time, they were uplifted by the knowledge that so many Australians had confidence in him and keenly wanted him to succeed.

Was that confidence well placed? Apart from his colleagues in the world of nuclear physics, who was to say? Yet so much of the University's allocation of funds, and so much of its reputation, were invested in this one project that people associated with its administration could hardly fail to be anxious, especially after the school abandoned the cyclo-synchrotron. Copland, some months after leaving Canberra, sounded out the Nobel Prize winning physicist Patrick Blackett, who commended Oliphant's courage and said he was keeping his fingers crossed about whether his machine would work. 'I didn't ask him whether the Birmingham thing had worked', Copland told Hohnen. 'It was a bit stupid of me not to have found out.'

In the meantime, other sections of the school were forging ahead. After remaining at Harwell for the first eight months of his appointment, Titterton arrived in Canberra in May 1951, and immediately started building his department of Nuclear Physics. It was assumed that the department would make use of the cyclo-synchrotron, once it
was completed. As an interim measure, the University imported a smaller accelerator of the Cockcroft – Walton type, allowing Titterton and his colleagues to get on with their research, which included photodisintegration studies, fast neutron experiments, and an intensive study of the energy level structure of an isotope of Beryllium. Early in 1952, the accelerator produced a nuclear reaction, stimulating great excitement within the University and among the press. The machine was in heavy demand, especially after the arrival of four PhD students in 1953, so the school decided to purchase another small accelerator, both as a source of spares for the larger machine and as a research tool in its own right. Titterton then acquired a third machine, an electron synchrotron which had been used at Harwell but was now surplus, as a gift from the British government to Australia. This supplied much more energy than the two existing machines, and gave the work of the department a powerful boost. Driven by Titterton's forceful (some said overbearing) personality, the department won acclaim for high productivity and turned out some outstanding students. Yet Titterton's reputation came in later years to rest more on his work outside the University, as an adviser to the British and Australian governments on atomic weapons testing.

Oliphant's third professorial appointment in the school was another former Cambridge man, John Conrad Jaeger, who came to the chair of Geophysics early in 1952. Geophysics was not on Oliphant's early list of priorities. A visit by the Canadian geophysicist J.T. Wilson excited his imagination as to what the area had to offer, and he determined to find someone to fill the chair, thus reversing the expectation often expressed by the Advisers that new fields should be opened up as men became available.

Geophysicists were in short supply, and Oliphant had two rejections before he approached Jaeger, who was not in fact a geophysicist at all. Rather he was, according to Oliphant, probably the finest applied mathematician in Australia. Born in Sydney in 1907, he had studied at the universities of Sydney and Cambridge before moving to the relative isolation of the University of Tasmania in 1936. His interests, spanning mathematics, theoretical physics and engineering, were unusually broad. While in Hobart he developed a highly productive collaboration with his former teacher Horatio Scot Carslaw, Professor of Pure and Applied Mathematics at Sydney for over thirty years, relating chiefly to operational methods in applied mathematics and the conduction of heat in solids. During the war he worked with CSIR scientists on aspects of radiophysics with direct implications for the war effort. At the time of his appointment to the ANU he was a hefty, bear-like man, with a large, round head and a genial but sometimes gruff manner. Although usually shy and retiring, he was a vigorous teacher and a forthright advocate of any cause he determined to pursue.

As this was the first chair of geophysics in an Australian university, Jaeger recognised a particular responsibility to develop the field. He saw geophysics as an extraordinarily wide subject, embracing the physics of the earth's atmosphere and its crust. In line with J.T. Wilson's recommendation, he decided to restrict work at the ANU to the crust and interior, leaving atmospheric research to the CSIRO and other institutions; but within that restriction he argued that, as the department was unique
in the country, it should offer postgraduate training in all aspects of geophysics. Research should therefore embrace the core subjects relating to the behaviour of the earth and its materials, namely mathematics, physics, petrology and crystal physics. He also insisted, contrary to Wilson’s advice, that research should extend outside the laboratory into the field, as there was no point in making physical measurements on rocks whose petrology and geological history were unknown.

The first research in the department grew out of Jaeger’s interest in heat flow in the earth and related chiefly to the cooling of mines and bore-holes. His earliest appointments, Germaine Joplin and Mervyn Paterson, opened up the study of petrology and crystal physics, and within a few years he had moved the department into rock magnetism, seismology and geochemistry. By 1960 he could claim, with reasonable accuracy, that the department was the most productive, not merely in the school, but in the whole University. As well as a steady stream of significant papers, there were several major achievements: the first determination of the thickness of the crust in Australia, making use of the atomic tests at Maralinga in South Australia in 1956; the first conclusive demonstration of continental drift, using palaeomagnetic observations on Australian rocks; the organisation (in association with the Sydney Metropolitan Water Board and the Snowy Mountains Hydro-Electric Authority) of a network of seismological observatories, which promised important engineering and geological results; the construction of very high pressure apparatus for deforming rocks, which opened the way to research of international quality in this area.

Most of these discoveries had prospects for practical applications. Jaeger was unashamedly an applied scientist, who turned the usual motivation for fundamental science on its head. He wanted to engage in ‘pure’ research in mathematics, physics and chemistry, in response to problems that arose in practice. He wanted to train geophysicists in fundamental science, but in areas directed towards later work in organisations such as the Bureau of Mineral Resources. He talked about ‘exploration geophysics’, meaning the study of the physical principles behind prospecting in the field. From the outset, he had his eye on ‘a very large audience out there’—politicians, mining companies, prospectors—who were aware of the potential value of geophysical research for exploiting Australia’s mineral resources. He also recognised that the department had the potential to attract external support, long before the quest for outside funding became part of the scientist’s job.

Some members of the University winced at Jaeger’s enthusiastic pursuit of practical applications. Oliphant, who held a purist’s view of fundamental research, expressed discomfort about where the department was headed; and Jaeger complained that he stood in the way of any incursion into the exploration side, ‘which in my view is the core of the subject and is the region where the money and the interest lie’. But he received plenty of encouragement from outside the University. H.G. Raggatt, Secretary of the Commonwealth Department of National Development, reassured him that research on the basic physics of the earth was essential if Australia was to discover new mineral reserves. What was wanted, he wrote, was fundamental knowledge about rocks and geological processes: there was no need to pursue economic objectives when conducting
research, as economic results would certainly follow.

Academics often turn out to surprise those who appointed them. Oliphant had expected Jaeger to work on one or two fields of geophysics, and could scarcely have imagined his determination to develop geophysics as he did. Jaeger had the foresight to recognise the potential role of geophysical research in Australia and the determination to carry it forward. He also saw that geophysics, if it was to advance as he believed it should, had to eschew narrow specialism and bring together a range of disciplines. As early as 1935 he suggested that the department be regarded as ‘an embryo School of Earth Sciences’, and in so doing signalled the beginnings of a long and sometimes stormy campaign.

If Geophysics was an outstanding success, the Department of Radiochemistry was, in Oliphant’s terms, a disaster. Frank Scarf, whom Oliphant imported from England to head the department at the level of Reader, found that his wife could not adjust to Canberra and was soon weighed down with health problems of his own. He produced nothing and gave no leadership; and although two or three of those nominally under him did good work, Radiochemistry as a department became an embarrassment. On Oliphant’s initiative, Scarf was encouraged to retire and the department was closed down, its other members divided between Geophysics and Particle Physics. If nothing else, Radiochemistry showed that departments were not immortal.

With the creation of the Department of Astronomy in 1950, the University gained an association with Mount Stromlo’s experienced and committed staff. Established in 1924, the Commonwealth Observatory antedated the University by many years, a point which later professors of astronomy were happy to make if ever the status of their department seemed under threat. Initially concerned with solar and geophysical work. Mount Stromlo had languished during the Depression. Woolley, who was appointed Commonwealth Astronomer and Director of the Observatory in 1939, promised to revive it and set it on a new course. Recognising the preponderance of observatories in the northern hemisphere and their scarcity in the south, he proposed to abandon the interests of the past and introduce stellar and galactic astrophysics. But war intervened, deflecting the astronomers towards designing and manufacturing precision optical instruments for the armed forces (which had the side benefits of increasing their number to about thirty and providing them with a well equipped workshop). The war over, Woolley turned Mount Stromlo towards the stars.

Woolley, who came from the classical British school of Cambridge and the Royal Observatory at Greenwich, was the first in a line of exceptional heads of the department. There was some dispute as to whether he was a great astronomer; but he was certainly a formidable presence, tall and imposing, with a sharp wit, plenty of style, and an extraordinary range of proficiencies, from folk-dancing to polo. He knew how to get things done, including extracting money and equipment from government and other sources. In 1948 he persuaded Chifley to part with £100,000 for the purchase of a 78-inch telescope. This was commissioned in 1955, and was at the time, along with a similar instrument in Pretoria, the largest telescope south of the equator.

Under Woolley’s direction, the Observatory focused on the galaxies known as the
Magellanic Clouds and the southern portion of the Milky Way. S.C.B. (Ben) Gascoigne and Gerald Kron, joined later by a visitor to the Observatory, Olin Eggen, discovered that some of the star clusters in the Magellanic Clouds were much bluer, and therefore much younger, than stars in our own galaxy. This provided the first clue that the Clouds had evolved differently from the galaxy, and helped explain galactic evolution. A Research Fellow, Gerard De Vaucouleurs, was able to show through small-scale photography that the Large Magellanic Cloud was a flat rotating system similar to the Milky Way. Most spectacular of all, a graduate student, Colin Gum, identified the remnant of the Vela supernova, which was named the Gum Nebula after its discoverer, who died too young to enjoy the fame due to his achievement.

Although the University’s annual reports claimed Astronomy as a department, and although the department produced the University’s first PhD graduate (Antoni Przybyski in 1954), the link was tenuous and dependent largely on Woolley’s status as honorary professor. Woolley wanted integration: he thought that the University would be a more congenial home for a research organisation than the Commonwealth Department of the Interior; he also held strongly that research should go arm in arm with teaching. There was some resistance inside and outside the Observatory, but Woolley, making use of his cordial relations with the Prime Minister, managed to get his way. Menzies told the University to proceed with the appointment of a full professor of astronomy, and in 1957 Mount Stromlo formally became part of the ANU.

By this time Woolley had returned to England to take up the position of Astronomer Royal and in due course receive a knighthood. His successor, Bart J. Bok, inherited a department and observatory ready to capitalise on the coming boom in astrophysics.
The Research School of Social Sciences

Geoffrey Sawyer, the newly appointed Professor of Law, hitched the caravan to the Vauxhall and the trailer to the Morris, and with his wife and two children set out in convoy on the two-day journey from Melbourne to Canberra. A lawyer needed no equipment, except books; and in Canberra he would be able to supplement his own library with the collections of the University Library, the National Library and the library in the Attorney-General’s Department. Shortly after arrival he was able to resume where he had left off, in the study of Australian constitutional and administrative law.

Sawyer’s intellectual baggage contained the elements of both a practising and an academic lawyer. While a barrister he had tutored part time at Ormond College in the University of Melbourne; and in 1939 he joined the University as a Senior Lecturer, where he taught courses in legal theory which drew on American as well as British texts. During the 1930s he took a close interest in the process of constitutional change which was shifting power from the states to the Commonwealth, and the evolution of administrative law to protect the individual against the actions of government. Although he immersed himself in technical aspects of the law, he also took a broad view of political and constitutional issues. As a divergent member of the Round Table movement, which sought to maintain links within the British Commonwealth, he spoke as an Australian nationalist and argued for greater Australian autonomy. As a friend and confidant of H.V. Evatt, he moved towards a political stance which was radical yet moderate.

His interest in legal theory was never far removed from what was happening in the parliaments and courts of Australia: for example, the application of the Westminster system in Australian parliaments; the composition of the High Court and the influence of different sorts of judges; the balance of authority between the Commonwealth and the states. Arriving in Canberra, he set to work on a mammoth project to record and analyse the relationship between federal politics and the law during the first half century of the Australian Commonwealth. The results were published in two volumes, the first in 1956 and the second in 1963. At the same time, he revised extensively his book on Australian Constitutional Cases, and researched and wrote on legal and political issues, ranging from hire purchase agreements and penalties to Australia’s role in the United Nations.

Like Sawyer, Noel Butlin knew exactly what he wanted to do. Back in Canberra in 1951 after his term as an Overseas Scholar, he was appointed Senior Research Fellow in the Department of Economics, and immediately threw himself into studying the structure of the Australian economy from 1860 onwards. Using the analytical techniques and informed by theories he had learnt at Harvard, he started gathering statistical data which showed the relationship between public and private capital formation and economic change. Soon the floor of his room was covered with mountains of statistical registers, yearbooks and every source of economic statistics he could lay his hands on for all the colonies and the first few decades of the Commonwealth. Nothing like this, even on a small scale, had ever been attempted in
Australia. The result was a completely new interpretation of the structure of Australian economic history. After hearing Butlin outline his central thesis at a conference in 1957, Brian Fitzpatrick, one of an earlier generation of economic historians, commented that it was masterful and fascinating, though it made him feel like ‘a pioneer who had survived to hear his own obituary’. After the publication of Butlin’s two books on investment and national income in 1962 and 1964, Australian economic history would never again be the same.

In contrast to Sawer and Butlin, Laurie Fitzhardinge was unsure of what he ought to be doing. He had been appointed Reader in the Sources of Australian History, a position suggested by Hancock. But Hancock was not around to spell out what the position ought to entail, and nobody else—least of all Copland—was prepared to guide him. Left to his own devices, he planned a survey of Australian manuscript sources, organised a seminar program on methodology in Australian history, edited one of the ‘First Fleet’ diaries, and delivered a paper to the Royal Australian Historical Society on the early political career of William Morris Hughes. The Hughes paper set the course for his main work over the next two decades. Hughes, then approaching the age of 90, attended the lecture and liked what he heard; and when in due course he saw the paper in published form, he immediately telephoned Fitzhardinge to say ‘You must drop everything you’re doing ... and get on to my life’. The next thing Fitzhardinge knew was that Hughes had arranged with Copland for a contract to be signed by which Hughes provided the University with modest support for research assistance and the University agreed that Fitzhardinge should write his life. ‘So that’, Fitzhardinge recalled 40 years later, ‘is how I got lumbered with Billy’.

Trained as a classicist and brought up in the tradition that history was one of the humanities, Fitzhardinge was never entirely comfortable in a school with the title ‘Social Sciences’. Mick Borrie, on the other hand, found that the garments of the social scientist fitted well. Before joining the University, he had taught social history to social workers in the University of Sydney. Early in his career, he had developed an interest in population studies, which he pursued as a Social Science Research Fellow. When he became Research Fellow in Demography, he went straight on with work already well under way.

Demography was a relatively new discipline. While courses in the subject had been introduced in some British and American institutions in the 1930s, chiefly in response to declining fertility rates in the west, Hancock was thinking ahead when he suggested it as one of his half-dozen growing points. As an area of research, it could be approached from various directions: history, biology, genetics, mathematics, statistics, human geography. Borrie came to it as a social historian, without a clear conception of the nature of demography as a separate discipline, but with an understanding that he should be concerned with qualitative research rather than the
quantitative analysis of statistics, which would best be left to official statistical agencies. Soon he was joined by Charles Price, whose background in history was similar to his own, and Norma McArthur, whose training in mathematics and genetics provided the formal statistical capacity the department needed, though it remained essentially a department of social demography.

In view of the post-war boom in immigration, Australia was a likely place for demography to flourish as a discipline. But its success at the ANU also owed much to Borrie’s ambition and achievement, and to the people he gathered around him. Demography proved to be a fine example of Hancock’s concept of intellectual development through growing points. In 1952 it was formally designated a department (though it had been one, in all but name, from the outset), and Borrie was appointed Reader and head. This made it the first Department of Demography in the world; and when Borrie was elevated to a chair in 1957, he became the world’s first Professor of Demography.

During its first decade, the department’s work ranged widely to embrace the structure of the Australian family, ethnic minorities in the nineteenth century, and the effects of post-war immigration on Australian society and culture. McArthur completed pathbreaking surveys of the growth and structure of the populations of the Pacific islands, before leaving in 1956 to direct the first thorough census in Fiji. Borrie set himself the ambitious task of researching and writing a demographic history of Australia. The European Peopling of Australasia eventually appeared in 1954; in the meantime he and his colleagues created the discipline of demography in Australia.

Statistics, like demography, was a relatively new academic discipline, having been established first at University College, London, in the 1930s, and introduced as a separate department at several other universities, including some in Australia, after the war. When Hancock initially suggested that the school should include a statistician, he had in mind someone who would fulfil a service function for his colleagues, though he might also have a large research project of his own. Eggleston went further, suggesting that the discipline might become a separate department if someone with appropriate interests in social statistics were available to head it.

K.C. Wheare located such a person in Patrick Moran, a young Australian who had trained at Sydney, Cambridge and Oxford, and who was now a lecturer in mathematics at Oxford. Moran had begun his career as a pure mathematician and had turned to statistics during the war. Rather than devoting himself to a single highly specialised line of research, as many statisticians tended to do, he had ranged widely across the field, developing the work of others and working closely with researchers in other disciplines. Copland, after meeting him in Oxford, doubted that he would
simply provide a service function to other departments, but thought he would prove a
stimulating and helpful colleague to anyone seeking statistical advice. According to
the Vice-Chancellor, he was also ‘highly suitable’ on other grounds: he had taken a
firm line on the debate about the Soviet scientist Trofim Lysenko (who advocated an
extreme version of Lamarckian evolutionary theory), and ‘seems to have got some
pleasure in jobbing the left wing scientists about their devotion to a group or party
that could perpetrate such intellectual rubbish’. Moran was duly appointed to a chair
(statisticians being in a seller’s market) and took up duty in Canberra in 1952.

Moran arrived with clear ideas about what he wanted to do. On the teaching side,
he expected to give lectures to economists and other social scientists about statistical
methods, such as the theory of sampling and sampling surveys. There should also be
courses with a different emphasis for biologists and medical scientists. On the
research side, he hoped to stimulate fundamental work in the theory of statistics.
Within a short time, he and his colleague E.J. Hannan, appointed as a Research
Fellow in 1953, were working on the prediction of sunspots, a general probability
theory of dams and storage systems, trend and seasonal variations in the value of sales
in Sydney clothing and drapery stores, and a remarkable range of other problems to
which statistical methods might be applied.

Eggleston’s plans for the school were taking shape. By early 1952 there were (if we
include Demography) six departments, three of them (Economics, Law and Statistics)
with chairs, and three (Demography, History and Political Science) headed by
Readers. Council had also reached a decision about the keystone of Eggleston’s
edifice, the chair of Social Philosophy. The occupant of this position, as Copland

Staff and scholars in the Research School of Social Sciences have a beer outside the tea
room, about 1957. Left to right Noel Butlin (supporting the verandah), Ross Martin
(Political Science), Don Rawson (Political Science), David Packer
(Demography), Warren Hogan (Economics), A.J.
Catt (Economics) and Ted Hannan (Statistics).
By courtesy of Joan Butlin.
explained it, was to explore the deeper problems of social structure, and to stimulate and guide his colleagues in the school, acting as ‘a sort of elder statesman’.

Partly because of the significance attached to it, this chair proved to be exceedingly difficult to fill. Eccles caused no end of trouble by proposing his friend Karl Popper, who was now Professor of Logic and Scientific Method at the London School of Economics. Although Popper had declined offers of several chairs in Britain, the Continent and New Zealand, and had refused to allow his name to be put forward for Cambridge, Eccles persuaded him that the ANU would offer him the opportunity to write the books that were in his mind, without losing his contacts throughout the world. Eccles was a powerful advocate, pressing Popper’s value as an integrating influence for the University as a whole. ‘With Popper on the campus’, he told Copland, ‘there would be little danger of our university becoming an assemblage of isolated departments. We would realise how much we held in common in our quest into the unknown.’

Coombs, Sawyer and Swan favoured the appointment, but others, including Wheare and Copland, were not so sure. Copland sought advice from all quarters, including some who knew Popper and his work and some who did not, and the more he heard, the more uncertain he became. The problem was that Popper was a controversialist, who attracted disciples and violent detractors wherever he went. Bertrand Russell, during a brief visit to Canberra, spoke highly of him, while some of Oxford’s younger philosophers said that his appointment would put the ANU on the philosophical map. Eggleston, after initial concerns that he might be one of those philosophers who evaded the great issues confronting mankind and took refuge in techniques which debunked reason and meaning, read The Open Society and its Enemies and came out in his favour. Others condemned his work as unscholarly and hinted that he was a charlatan, or suggested that he was really a logician and therefore unsuited to a position in Social Philosophy. Florey, who tended not to like Jews unless he happened to know them, warned Wheare that such types should be avoided:

... I know absolutely nothing about him, though I have views about this sort of thing on general grounds. I have no doubt that he is of Central European origin, and I feel very strongly that it would be a thousand pities if the University is loaded up with these people, who tend to throw their weight about quite unnecessarily and, in a place that is growing and will undoubtedly have troubles, are likely to be intolerable nuisances.

This said much about Florey and nothing about Popper, and would be irrelevant except that Florey’s prestige made his views, mediated through Wheare, inevitably influence the outcome. After six months’ agonising, Council reached a compromise decision by which Popper would be invited to visit Canberra for six months, sufficient time to assess his suitability for a permanent appointment. In the event, Popper could not come for longer than three months, and Council seized the opportunity to let the offer lapse.

Copland was evidently relieved. It was all very well to be adventurous in making appointments, but Popper threatened more adventure than the University was ready for. First, there was a question mark hanging over his scholarship: if, as time passed,
the most commonly heard answer to the question turned out to be ‘no’, the University’s academic prestige would be compromised. Equally dangerous was the prospect of his becoming a focus of division. As John Passmore, an Australian who held the chair of Philosophy at Otago, told Hohnen, Popper lacked academic manners: one had to put up with being interrupted, misunderstood, prevented from getting a word in, and so on. Passmore thought this defect was compensated by the quality of what he had to say; but others thought him insufferable.

All in all, as the University was translated from paper to reality, it was not becoming easier to make appointments. There was now an additional criterion of selection: new appointments had to fit in with those who were already there. As Sawyer put it, they had to be ‘clubbable’. According to Wheare, Popper was a great success in Britain and the United States, where he could bombard people with his ideas. But would Canberra survive the onslaught? Those who were already there determined that, unless they were allowed an obligation-free trial, the risk was too great.

With the Popper diversion out of the way, and after tossing up whether the chair should be renamed simply ‘Philosophy’ but deciding to stay with Eggleston’s title, Council went on to appoint P.H. Partridge. Now in his early forties, Partridge had studied and trained under John Anderson at the University of Sydney, where his chief interest had been in epistemology. From the late 1930s, he moved towards ethics and political theory and he was now Professor of Government and Public Administration at the University of Sydney. The blending of a philosophical background and current interests made him well suited to the role Eggleston had in mind. Another attraction was his reputation as an inspired teacher and outstanding administrator.

Partridge regarded Social Philosophy as akin to Political and Social Theory, as it was taught, for example, at Oxford. He defined the field as ‘the examination of the more general and fundamental conceptions, the more general assumptions and principles, which are employed both in the several social sciences and in the thinking of ordinary men about the organization of political and social life’. If this was to be the main work of the department, he thought it needed to be buttressed by at least one philosopher who was thoroughly abreast with developments in the fundamental branches of philosophy, namely logic, epistemology, metaphysics, and the history of philosophy. Such a person was his former colleague and fellow Andersonian, John Passmore, who came across from Otago in 1955 as Reader in Philosophy.

Partridge and Passmore, along with their untenured staff and students, comprised as strong a team as was gathered anywhere in the University. Partridge began work on theories of freedom, with specific reference to recent history, and published ‘think pieces’ on such subjects as ‘The rights of the citizen’, ‘The Australian universities and governments’ and ‘Value judgements in the social sciences’. Passmore, after almost a year in Oxford, arrived in Australia to complete what turned out to be a seminal work on A Hundred Years of Philosophy, published in 1957. In other papers, he considered historical and methodological themes as well as problems in modern philosophy. Before long the buttress of ‘pure philosophy’ was as strong as the social philosophy structure it was intended to support.
The Research School of Pacific Studies

Patrick FitzGerald was preceded to Canberra by several crates of the books he had bought in Hong Kong. His first job was to unpack and arrange them on the shelves of his office in the old hospital building. Then he had to find staff with expertise in Chinese or Japanese history, which proved to be no easy task.

FitzGerald intended to range across nearly three millennia of East Asian history. The Far Eastern historian, he argued, had to be more of an all-round scholar than would be thought necessary in fields where workers were numerous and the sources easy to come by. In his own research, he moved with ease between classical Chinese history and the tumultuous events in the country he had just left. Soon after arriving in Canberra he published Revolution in China, which was later published in Penguin Books as The Birth of Communist China. Then came a biography of the seventh-century Empress Wu, followed by Flood Tide in China, which explored the first few years of the Maoist regime. In the early 1960s he was working on the origins and use of the chair, or ‘barbarian bed’, in China, as well as writing and presenting papers on contemporary Chinese politics and international relations. Both tasks were part of his mission to understand China and interpret the Chinese mind to western audiences, at a time when Australian government policy towards the People’s Republic amounted to little more than wishing it would go away. In 1956 he led an eleven-member unofficial cultural delegation (including Partridge) to China, which improved the prospects for academic exchanges between the two countries.

Although the government remained suspicious of FitzGerald’s political outlook, the University quickly acknowledged his attainments, especially after the School of Oriental and African Studies in London tried to tempt him away with the offer of an associate professorship. His ‘Visiting’ status was dropped, and he was asked to take charge of a separate Department of Far Eastern History, a name chosen in preference to ‘Oriental history’, but which yielded to tradition in locating China and Japan to the east of England. Soon he was appointed to a chair, which went most of the way towards removing any seeming illegitimacy implicit in his want of a degree; the award in 1968 (shortly after his retirement) of a DLitt, on the basis of published works, dissolved the stain entirely. By that time he had created, almost single-handed, Sinology in Australia.

Jim Davidson plunged into Pacific research with an enthusiasm that was comparable with FitzGerald’s and which suggested a more than merely academic love for his subject matter. According to his long-term friend and departmental colleague Harry Maude, his initial interest was based on a frankly romantic attraction towards the South Seas, which
his mother had instilled in him since childhood. During frequent field trips he
developed an affinity with the peoples whose history he was studying; in Western
Samoa, wrote Maude, ‘he passed with honours his apprenticeship in the subtle nuance
of island life and acquired a sensitive perception of the ethos of the islander which so
many Europeans have sought and so few have gained’.

Davidson intended the Department of Pacific History (once Fitzgerald and his
colleagues had broken away) to cover the Pacific islands fairly comprehensively and to
enter South-east Asia selectively, as opportunities for comparative research arose. By
1960 he and his colleagues had conducted research on Western Samoa, Fiji, Tonga,
the Cook Islands, French Polynesia, New Guinea, Malaya and Indonesia. Davidson
believed that the exact location of research did not much matter, so long as the work
illuminated problems relating to the contact between western and indigenous
cultures. Where previous researchers had tended to approach the Pacific from the
perspective of imperial policy-makers, Davidson saw the islands through the eyes of
the colonised as well as the colonisers. This approach led him into other disciplines—
anthropology, sociology, linguistics—and it constituted his own and his department’s
special contribution to Pacific studies.

When he was not editing the journals of Captain Cook or following the exploits of
the early nineteenth-century adventurer Peter Dillon, Davidson was engaged in a long-term
study of how political institutions in Western Samoa had evolved since the coming of the
Europeans. As the colony moved towards independence from Britain, his services were
eagerly sought by the Western Samoan government, and between 1959 and 1961 he
spent several months in Apia as Constitutional Adviser. In his view this was an
appropriate extension of the work of a Pacific historian; but the Vice-Chancellor at the
time, Sir Leslie Melville, was not so sure, commenting that ‘While he is certainly taking
part in the creation of history, such activities are perhaps marginal as academic activities
of a History Professor’. In Melville’s estimation, a professor’s worth was measured by
his publications; and Davidson’s promised books were slow in coming. When *Samoa mo
Samoan: the Emergence of the Independent State of Western Samoa*, eventually appeared in
1967, it offered convincing evidence of the historian’s role as the link between past and
present, and confirmed that his time in Apia had been well spent.

The Pacific Historians (or most of them) were linked by a theme. The Geographers,
on the other hand, ranged widely across their discipline and were not even particularly
obedient to regional constraints. Oskar Spate, who arrived as Professor of Geography
in 1951, was a polymath whose interests knew no mental or geographical boundaries;
and given that geography had made little headway in Australian universities (at the
time he took up duty there was only one other chair), he thought the ANU department
should try to develop as many facets of the discipline as possible. This implied striking
a balance between the physical and human sides of the subject, pure and applied
research, and systematic and regional studies.

A geography department above all ought to have been comfortable in a regional
school. But there was a problem, which Spate expressed with characteristic whimsy:
‘geography is the study of man and land in their relations, and there is not really much
of either in the Pacific'. While it was hard to attract students to work on the Pacific islands, there were plenty of people eager to work on Australian geography and there was much important work to be done. So Spate adopted a frankly opportunistic policy of encouraging research on the mainland, while seizing every chance to work in the Pacific islands, South-east Asia, and especially New Guinea.

Like the Pacific Historians, the Geographers spent much of their time in the field. In the mid-1950s staff and students in the department were studying coastal geomorphology on King Island, tracing the steps of early European explorers along the Birdsville track, investigating irrigation geography along the Murray River, and conducting regional surveys in Samoa, the New Hebrides, New Guinea and Sumatra. Fieldwork was often arduous and often an adventure: the department's report for 1956 recorded, with nice understatement, that a student's research in central Sumatra 'was to some extent protracted by the unusual prevalence of tigers in her area, which made work away from the villages somewhat hazardous'.

Spate's own research interests, as they developed over the next few decades, extended from Burma (where he had lived before and during the war) to Chile. He brought with him to Canberra a near-complete manuscript of a geography of India and Pakistan, which was published in 1954 and soon became a standard work, in Russian as well as English. While maintaining an interest in the subcontinent, he embarked on a study of the Australian cultural landscape, made several forays into New Guinea and conducted, at the request of the Fijian government, an enquiry in 1958 into the economic problems of the ethnic Fijians. Although he maintained a steady stream of publications and presented many conference papers, he later described much of this early work as scholarship rather than research, as so much of his time was taken up in encouraging the work of others. His own greatest work was yet to come.

International Relations had troubled beginnings. When Crocker left for New Delhi in 1952, the small department was placed in the charge of Michael Lindsay, a Senior Research Fellow who was soon promoted to the permanent position of Senior Fellow. Lindsay, son of the distinguished Master of Balliol College, Lord Lindsay of Birker, was an expert on contemporary China, having taught in Beijing in the late 1930s and early 1940s. During the war he had served alongside Mao's armies against the Japanese occupation and had expressed strong support for the communist cause. This effectively disqualified him for a university job in Britain; so his father wrote to his old friend and student Crocker about finding him a job at the ANU. As it happened, by the time he arrived in Canberra Lindsay had reversed his previous opinions and had become a fervent critic of the Maoist regime.

On Crocker's resignation in 1954, Lindsay (now Lord Lindsay) applied unsuccessfully for the chair, which remained unfilled. As the University seemed reluctant to promote him even to the readership that he insisted had been promised, he came to see himself as the victim of an injustice and made his grievances publicly known, questioning the University's standards in social science research, as well as its appointment and promotion procedures.

In 1957 the department's problems looked as though they might be solved when Martin Wight, an outstanding scholar from the London School of Economics,
accepted the chair and reached the stage of passing a medical examination. But Lindsay wrote to him in terms that scared him away. Soon the case was completely out of hand, with Lindsay writing scathing articles for the Sydney Observer and lambasting the University on ABC television. In the meantime, the department languished. While individual members, including Lindsay himself, continued to do valuable work, there was nothing and no-one to hold it together.

Anthropology, on the other hand, demonstrated a remarkable cohesion from the outset. Apart from the vibrancy of the discipline and the fact that four of its early staff, along with Firth, the Adviser to the school, were products of the London School of Economics, it owed much to Fred Nadel’s intellectual leadership and his sense of urgency about all the jobs that needed to be done. Within a few weeks of his arrival in Canberra he had set out a detailed research program, comprising projects on social organisation in the New Guinea Highlands, social and cultural change in the Pacific islands (with emphasis on the appearance of the ‘cargo cult’), a Pacific community responding to western influence, village communities in Indonesia or the subcontinent, and the process of assimilation among recent European immigrants to Australia. The last of these, he conceded, was not strictly ‘anthropological’ in the conventional sense of the word, but that did not matter: there was no sharp distinction between anthropology and sociology, and the anthropologist ought to be concerned with both ‘primitive’ and ‘advanced’ cultures. There was as yet no chair in Sociology in an Australian university, and Nadel was keen to stake a claim. At his request (and despite some mutterings about empire-building), the department was soon renamed to embrace both Anthropology and Sociology.

Where Spate allowed individual preferences to determine research directions, Nadel believed that the continuity of research and the extension of knowledge were best served by connected and well-planned programs. He favoured teamwork; and before long a group of students was hard at work on comparable studies of several communities in New Guinea and New Britain. This association of interests provided the basis for a lively seminar program.

The department also developed interests in Aboriginal anthropology, although for a decade Bill (W.E.H.) Stanner was the only staff member in the field, and only two of the first dozen students conducted research on Aboriginal themes. In the 1950s, Stanner returned to places in the Northern Territory that he had visited nearly twenty years earlier and recorded the drastic changes that had taken place there. His observations led him to question the adequacy of current anthropological theories: ‘We have men in anthropology like those trees in whose shade nothing will grow. In a sense we should be glad of it. It gives a relish to radical thinking.’
thought radically about continuity and change in Aboriginal societies, and wrote profoundly about Aboriginal religion, including that ‘impalpable and subtle’ concept, The Dreaming. Not many people, beyond his professional colleagues, listened. It was well into the 1960s before his work started to attract the attention it deserved.

Nadel’s own work culminated early in 1956, when he posted to his publishers a major book on The Theory of Social Structure. A week later, suddenly and unexpectedly, he died of a heart attack, aged just 52. His colleague Derek Freeman wrote: ‘By the tragic untimeliness of his death anthropology has lost incalculably’. But the department he created was already strong enough to withstand the blow.

The creative imagination

By 1953 there were sufficient senior academics on campus for the University to offer a public lecture series. The breadth of subject matter was impressive: Woolley on ‘Ancient and modern theories of the universe’, Borrie on ‘The growth of populations’, Sawer on ‘Law as logic and common sense’, Oliphant on ‘The methods of modern physics’, Nadel on ‘National character’, Davidson and Partridge debating ‘The uses of history’. The University was taking shape as the intellectual community envisaged by its founders.

There was a sense of intimacy within the University, a result partly of its members being pioneers together, and partly of their numbers being small. As in Oxbridge colleges, people from different schools and disciplines were thrown into contact with one another, confirming the impression of a community of scholars. Moran in Statistics worked with Steven Fazekas de St Groth in Microbiology to develop a mathematical theory relating to host resistance to virus infection. Jaeger in Geophysics helped Eccles with his work on nerve cells. The Department of Social Philosophy conducted a seminar on methodology in the social sciences which was attended by staff and students from several departments in Pacific Studies and Social Sciences. The professors from all four schools met for a formal dinner three times a year. Staff who kept to themselves during working hours might well meet their colleagues at social gatherings or the shopping centre. Most staff lived within walking or pedalling distance of the campus, so the chances of casual contacts between the disciplines were high.

University House, when it opened in 1954, served, as Hancock intended it should, as a universal meeting place, especially at lunchtimes, when staff, scholars and visitors from all schools and the administration were thrown together indiscriminately in the hall. The first Master, Dale Trendall, was a New Zealander, aged 44, with a fine appreciation of Oxbridge (in his case, Trinity College, Cambridge) collegiate life. Coming to the ANU from the chairs of Greek and Archaeology at Sydney, where he had also occupied various senior administrative positions, Trendall was an urbane scholar, with cultivated tastes and a cutting wit. He was also an internationally acknowledged expert on South Italian Greek vases, which brought him dangerously close to Wright’s collector of tailored tweeds, but fashioned him well for the integrating function the Master was expected to perform.
But despite all the favourable signs, some of those involved in setting up the framework of the University were starting to have doubts, especially in relation to the social science schools. Alf Conlon decided as early as October 1950 that these schools were ‘irreparably stuff’. His opinion by this time did not matter, but Oliphant’s did; and he, isolated as the only Director and frustrated by his building problems, was ‘appalled’ by his colleagues’ lack of academic experience and appreciation of what a university ought to be about. In his view, the schools were developing as watertight compartments, each pursuing its own interests, without a common objective. The problem, he said, was acute in the social science schools, where too many staff had come from public service backgrounds, with the result that the University was starting to be run like a government department. His answer was to forget the past and appoint Hancock, who had the capacity to turn the place into ‘a real university’: an institution which embodied the traditional academic ideal of a group of men with a common aim and a common spirit.

Oliphant expressed these misgivings to Eggleston, who defended the social science schools on the grounds of their experimental nature, but nevertheless picked up the cue. Early in 1951, while still a member of the Interim Council, he warned in a testy memorandum that the social science schools were developing without a sense of direction: ‘those who commence work in Canberra apparently take up the subject which, for the time being, appeals to them as appropriate’. There was no formal provision for cooperative research, with the result that academics could engage in ‘an orgy of fact finding’ in their own discrete disciplines. Renewing his correspondence with Hancock, he wrote that it was essential to forget the accidents of history and differences in methodology which kept the disciplines apart, to get away from ‘narrow specialisation and academic evasion’, and to encourage researchers to explore the fundamental problems of society. ‘Apparently I was wrong to think that the schools of research at the National University could help in producing these men, but that was my dream.’ Assuming Hancock received this letter, we can imagine him reflecting wryly on his earlier premonitions.

Nobody in the social sciences took much notice of Eggleston’s warnings. Sawyer had just finished writing a report on the manuscript of his Reflections of an Australian Liberal, praising it sufficiently for publication, but denouncing it to himself as ‘a shallow piece of pro-democratic propaganda masquerading as sociology ... I agree heartily with the objects of the propaganda, but detest the half-baked identification of wish with facts.’ Swan, whom Eggleston had privately reproached for pursuing subjects of purely technical interest, argued (in another context) that the study of techniques was a legitimate object of research and appealed for funds to collect empirical data, since a school of social sciences which relied on published statistics would be severely handicapped.

This was Eggleston’s last attempt to influence the way the University was headed. He did not seek membership of the new, permanent Council convened later that year, but continued to contemplate the University and the world with growing disillusionment from his wheelchair in Melbourne until his death in 1954. It seemed that Eggleston was yesterday’s man; but in some senses, he was also tomorrow’s.
Nevertheless, there were signs that some members of staff were aware of a loss of direction, especially in Pacific Studies. The school’s Annual Report for 1953 remarked that interest in staff seminars, in which people from the two schools expounded their research, was waning, while enthusiasm for interdisciplinary training courses was more theoretical than practical. The report conceded that, while there was rapid expansion, there was as yet little trace of coordinated planning: ‘rather, centrifugal tendencies predominated, concentrated attacks upon related problems giving way before diversity of research’.

Coombs, though similarly troubled, was more reticent than Eggleston in expressing his reservations. Publicly at least, he kept his peace until 1957, when he seized on continuing uncertainty in the Department of International Relations to press for a reexamination of the role of its school within the University. The present organisation of Pacific Studies, he told the Vice-Chancellor, does not take advantage of ‘the unique opportunities for co-ordinated studies of problems which arise in the Pacific of importance to Australia’. In other words, the academics were concentrating too narrowly on problems within their own disciplines at the expense of a broader concern with meeting national needs. The school took this warning to heart, and undertook a broad review of where it was headed.

But Coombs knew better than to force the issue, remembering perhaps what Wright liked to call Conlon’s ‘law of social institutions’: ‘Every such institution comes to the level of the society in which it is and bears no essential relationship to the expressed ideals of the people who founded it’. Wright repeated this dictum from time to time with varying emphases and degrees of relish. But it was Coombs who refined the idea (perhaps it was as much his own as Conlon’s) and gave it most forceful expression when he delivered over ABC Radio the Boyer Lectures for 1970, which he called ‘The fragile pattern: institutions and man’. By now he was (almost) reconciled to what the ANU had become, ‘a university of quality and distinction, but bearing little resemblance to the ante-natal image of its parents’. The concerns which he had shared with Wright and Conlon, Walker and Mills, and the rest of the post-war planners were not shared by the newly appointed staff.

Scholars were disinclined to direct their labours to policy objectives, which they felt to be parochial and earthbound; the problems which excited their curiosity and imagination were intellectually rather than practically motivated; they were anxious to establish their identity with their colleagues in other places and in other times rather than with the eager re-builders of contemporary society.

This, said Coombs, was as it should be: the investigator ‘can no more be guided or directed than can the artist.’ The days of social engineering had passed, and the creative imagination had to be given free rein.

Ironically, as Coombs was well aware, the freedom assumed by scholars at the ANU to pursue their own research interests was one of the marks of a genuine university.