The Cavendish Tradition in Australian Physics — Time for Change

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The Oxbridge influence was paramount in Australian Universities from the beginning; the coat-of-arms of the University of Sydney was composed symbolically of the open book of Oxford and the lion of Cambridge. Furthermore, two of the first three professorial appointments at Sydney University were scientists, including Morris Pell, Senior Wrangler at Cambridge in 1849 and Fellow of St. John’s College, to the Chair of Mathematics and Natural Philosophy. At the University of Melbourne, which began teaching three years later in 1855, a professor of Natural Philosophy was not appointed until 1882, and even then the choice was limited to the only candidate for the chair, Henry Andrew, who did, however, have a Cambridge education.

The University of Adelaide was blessed by the appointment in 1875 of Horace Lamb, Fellow of Trinity College, Cambridge, as its first Professor of Mathematics. Lamb had been taught by Stokes and Maxwell and was familiar with the work of William Thomson as well as that of the European theorists. The Cavendish influence thus came into Australian physics from the very beginning.

There is no single, unique or even well-defined Cavendish tradition. Pippard (1974) has pointed out that “Cambridge was not the first university in the British Isles to encourage experimental physics, and if in its earliest years the Cavendish stood out from the rest, it was more because of its first two professors, Maxwell and Rayleigh, than for any notable difference in outlook.” “Only after this, with the inspired appointment of J.J. Thomson at the age of 27, did the work begin to develop a special character.” This character, which Thomson inspired during his 35-year occupancy of the Cavendish Chair, further developed and evolved under the leadership of Ernest Rutherford, who stayed from 1919 until his death 18 years later. It was the Cavendish of Thomson and Rutherford that had an especially powerful influence on Australian physics, and it is to the impact of that particular tradition that I want to refer.

This began in the 1880s when the second and most outstanding generation of physics professors were appointed to the three Australian universities: Richard Threlfall (to Sydney) and William Bragg (to Adelaide), both of whom arrived in 1886 with youth and impeccable Cambridge records, and Thomas Lyle, who sailed to Melbourne from Trinity College, Dublin in 1889. Threlfall was a close friend of J.J. Thomson and, after his return to England 1898, “became an important link between J.J. and military and industrial science” (Crowther, 1974).

It was unlikely that this triumvirate would be quickly equalled in later years, but on an individual basis Lyle’s successor in Melbourne, Thomas Laby, outshone them all in terms of the contribution that he and his department made to Australian Physics. Laby, too, was a Cavendish graduate.

With this brief introduction I now turn to the several examples I have chosen to illustrate my theme.

Bragg in Adelaide

William Henry Bragg graduated as Third Wrangler from Part II of the Cambridge Mathematics Tripos in 1884 and with first-class honours from Part III in 1885. During his later months at Cambridge Bragg attended some of J.J. Thomson’s lectures, spent time in the Cavendish and played tennis regularly with the professor. It was “J.J.” who suggested that he apply for the Adelaide Chair, which extended to cover physics as well as mathematics and which he took up early in 1886.

The outline of Bragg’s subsequent career in Adelaide is well known, although the existing literature represents only very poorly the academic contributions he made in his first 17 years there. It is on Bragg’s subsequent investigations of the nature of X- and γ-rays, however, that I wish to focus, and particularly on his vigorous disagreement with Barkla on this question; the first cause célèbre in the history of Australian physics (Stuewer, 1971).

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Fig. 1: Rutherford and Thomson watching an annual Cavendish cricket match (from the Cavendish Collection. with permission).

Fig. 2: Todd family, Adelaide 1897. W.H. Bragg second from left back row, his wife Gwendoline (nee Todd) second from left centre row, and their two sons (William Lawrence and Robert Charles) front row (from the South Australian Archives Collection, Ref. B28760, with permission).
From the beginning the Cambridge physicists were mechanical in their outlook, and this applied to their theoretical as well as their experimental investigations. Maxwell's early writings on electricity and magnetism were based on the hypothesis that electric and magnetic phenomena could be understood in terms of the actions of a mechanical medium; ultimately (but only outwardly) the models disappeared altogether and only the mathematics remained. This tradition was actively carried forward by Thomson. We all remember his "plum-pudding" model of the atom, whose value, however, is commonly underrated and whose nature is often misunderstood. He deplored the great tendency among students to regard Maxwell's theory as a set of differential equations instead of attempting a "mental picture" of what was taking place. Thomson strove constantly for the concrete image as the starting point for all physical understanding (McCormmach, 1967).

Some of these powerful influences were surely at work when Bragg suggested in Adelaide in 1907 that X-rays and γ-rays consisted of material particles. Others had made a similar suggestion, but Bragg's neutral-pair hypothesis, which pictured high-energy radiation as an alpha or positive particle together with a beta or negative particle, became the focus of this point of view. It was a natural outgrowth of his earlier radioactivity studies.

Such a particle pair would have great penetrating but weak ionizing powers, be influenced by magnetic or electric fields and show no refraction. Furthermore, Bragg noted that in a violent encounter with matter such a neutral pair would be resolved into separate positive and negative particles, the beta particle appearing in the forward direction as a secondary ray.

X-rays behaved somewhat differently. In his earlier X-ray experiments, Charles Barkla found no such asymmetry in the scattered X-rays, a result in accord with the ether-pulse theories of Stokes, Weichert and Thomson, where X-rays were pictured as a stream of independently-moving, transverse electromagnetic waves or pulses. It was not surprising therefore that Bragg's corpuscular model brought a sharp response, particularly as Bragg stated that the evidence for the pulse theory was "indirect" and "a little over-rated". How could spreading pulses, for example, concentrate enough energy on one atom to ionize it in the first place, Bragg asked.

We can now see that the two angular distributions that were being measured were quite different: Barkla primarily of Thomson-scattered soft X-rays, Bragg largely the forward-peaked Compton-recoil electrons; but the dispute continued without this benefit of hindsight. In 1908 Bragg altered the alpha-electron pair to a positive-negative electron pair, and in 1910 to an electron and a positive particle "which adds little to its mass"; in 1909 he had returned to England.

The discovery of X-ray diffraction in 1912 did not deflect Bragg from his primary belief, for in 1913 he wrote: "The problem remains to discover how two hypotheses so different in appearance can be so closely linked together". It was yet another decade before Compton, Bohr, Heisenberg and others provided a solution; and it is fitting that it was Bragg who then distilled the essence of the matter in his famous remark that physicists use the wave theory on Mondays, Wednesdays and Fridays, and the particle theory on Tuesdays, Thursdays and Saturdays.

Laby in Melbourne

The life of Thomas Howell Laby is sufficiently well known to require little elaboration here. The reward for his early exceptional independence and originality was the award to him in 1904 of the Sydney 1851 Exhibition Science Research Scholarship.

After consultations with Ramsay in London and Poynting in Birmingham, Laby took up his scholarship in Thomson's Cavendish Laboratory. The research he undertook, the experiences he enjoyed and the friends he made there profoundly influenced his life. His work also brought him into contact with Rutherford, with whom he formed a life-long friendship. This was also important, for Laby later sent a stream of postgraduate students to the Cavendish in Rutherford's time, and it also kept him in constant touch with developments in physics overseas. During the twenty years between the World Wars no less than 12 of the 21 1851 Scholarships awarded to Melbourne University went to Laby's students, and all of these men (plus Massey and several others) undertook their studies at Cambridge. Earlier, Bragg had sent his two best research students (Keelem and Glasson) there on 1851 Scholarships, and Laby had sent two from Wellington in New Zealand (Burbridge and Hercus); thus was the Cavendish connection continuously cemented. By contrast, not a single Australian, to my knowledge, joined Bohr's Institute in Copenhagen, then the world centre for ambitious young theoreticians.

Laby succeeded Lyle as Professor of Natural Philosophy in the University of Melbourne in March 1913; and in one particular and noteworthy aspect of Laby's work there, his basic and practical upbringing was more influential than his Cambridge education. I refer to the two-pronged character of much of his studies: research for its own sake, but also research into the physics of problems of a very practical nature. I want here to centre particularly on Laby's most active participation in the development and use of X-ray spectroscopy for the quantitative chemical analysis of practical samples, and on his consequent dispute with Hevesy, the second cause celebre in the history of Australian physics.

It was towards the end of the 1920s that Laby, Eddy and Turner turned their attention to quantitative chemical or elemental analysis, a problem that had been brought to their attention by Dr. Ian Wark of the Electrolytic Zinc Company of Australia (1929). They listed the advantages of X-ray emission spectroscopy and suggested that the doubts thrown on the method by other recent experiments that had provided erroneous identifications were unjustified, since such errors could be avoided by a more careful experimental approach. The major problem remaining was one of making the technique quantitative rather than qualitative. But before Laby could report further on the work, the atmosphere suddenly became rather foggy, due to the arrival of a paper by Gyorgy Hevesy, a most prominent and respected member of the scientific community and a formidable opponent.

Hevesy's Johannesberg paper (1929) pointed out that he and Coster had, in their hafnium work, used quantitative X-ray spectroscopy to test the efficiency of a chemical separation method. In cases other than refractory oxides, however, Hevesy suggested, appreciable difficulties were encountered. Many of the errors induced could be eliminated, he continued, by exciting the sample not by electrons in an X-ray tube but rather by allowing X-rays from a standard tube to irradiate the sample; the secondary

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rather than primary X-rays then being analyzed. Hevesy had discovered the technique of X-ray fluorescence analysis.

Laby and Eddy were not to be so easily cast aside, however; during 1930 they would publish four papers in strong support of the value of the “primary” method. The experimental difficulties were considerable, they conceded, but, as they had previously demonstrated, a much higher sensitivity than found by others was possible. Hevesy responded: “I must ... entirely disagree with their statement that the entire X-ray spectrum of an element can be obtained (particularly) at concentrations less than 0.0001 per cent,” and he reported the accusation that Laby and Eddy’s method was only successful in “special cases”.

The next volley was fired by Laby in the May 31 (1930) issue of Nature. He admitted that the “primary” method was not yet as sensitive as previously stated for non-metals; and then, turning from defense to attack: “Has Prof. Hevesy evidence that the sensitiveness mentioned cannot be obtained with a metal?”

At the end, both Laby and Hevesy were correct. Hevesy’s X-ray fluorescence spectroscopy certainly became an analytical tool of very wide-ranging use. But Laby also established convincingly that, with his care and precision, it was possible to achieve the sensitivity which he claimed. In recent years, the advent of new levels of instrumental and theoretical sophistication have prompted renewed interest in this formerly discarded method, not for elemental analysis but for studying the electronic properties of metals and alloys. Laby would surely have been pleased.

Rutherfordian Influences

I now want to make a number of remarks arising from my own experiences in Australian physics, dating from the late 1950s. I want to suggest that the Cavendish-Rutherford influence has been a major significance, and that this influence has not always been a healthy one. Like us all, Rutherford too was a product of his time.

Most of his firm views and beliefs, which he saw no reason to hide, were not appropriate for the early decades of this century; the few that were, were laughed off, for Rutherford’s depreciations or deprecations were usually expressed with a twinkle in the words, in the voice and in the eye. But Rutherford, through the multitude of his students and colleagues, exercised an enormous influence on the world-wide development of physics (including Australia), and his prestige was such that a casual remark from Rutherford became a dogma in lesser men’s minds. Rutherford died in 1937 when the Second World War was just around the corner. As so many of his Cavendish colleagues pointed out, it was the end of an epoch; but this message took longer to reach the antipodes. Let me illustrate with four examples.

(a) Applied Physics

With regard to the Cavendish of J.J. Thomson, Crowther has stated (perhaps overstated): “The professor and the staff had never had anything to do with the application of science, in industry or in war. They had pursued physics as part of the cultural equipment of nineteenth and twentieth century educated society” (Crowther, 1974). Kapitza has said: “Rutherford did not take any interest in technical problems and I even had the impression that he was prejudiced about applied problems.” C.P. Snow (1965) has written: “Pure scientists have by and large been dim-witted about engineers and applied science. They couldn’t get interested. They wouldn’t recognise that many of the problems were as intellectually exacting as pure problems, and that many of the solutions were as satisfying and beautiful. Their instinct was that applied science was an occupation for second-rate minds.” Rutherford was attacked personally from time to time on this matter; nor am I convinced by Oliphant’s rejoinder that “these criticisms have been seen to be baseless when former research students from the Cavendish Laboratory contributed so much to central wartime developments...” (Oliphant, 1982). What happened in the exceptional circumstances of two World Wars is hardly the point.

Thomson and Rutherford had little need to apologize; their laboratories specialised in pure research for its own sake, and did it superbly well. It is quite another question, however, as to whether all the universities in Australia should attempt exclusively to follow that lead. I need not detail in this paper, I hope, that the Americans, for example, have adopted a substantially different point of view, to their credit and to their great advantage.

That Australia followed the Cambridge tradition quite religiously is nowhere better demonstrated than in the recent extensive and passionate writings of Susan Davies (1981) on the setting up and preliminary discussions of the 1964-65 Martin Committee on the Future of Tertiary Education in Australia; a work which provided the initial inspiration for the present paper, which I gratefully acknowledge. Martin agreed wholeheartedly with Cockcroft, who called for the establishment of institutes of technology separate from the universities, and with the editors of Nature, who asked, in April 1961: “Would it not be of greater advantage to teach technology elsewhere rather than at a university?” It was no accident that Monash University was originally conceived as an institution to play such a role. After all, it was Martin who said in an interview in 1975: “Now, the education that I am aware of as a professor of physics (at Melbourne University) was good for turning out men who were going to do research—the best of them—and, of course, there would be the rump who would come out as B.Sc’s and become teachers or turn into engineers or what have you”.

Also significant is the fact that when Brian Pippard delivered his Inaugural Lecture as Cavendish Professor

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* This is, however, the method used in the electron microprobe analyzer. (Ed.)

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in October 1971 he pleaded for a change in direction of Cavendish research. He pointed out that physics had suffered a decline in prestige, something that would not have occurred if physicists had paid more attention to the needs of society, and their duty to play a more positive role in it. Physics is not, he went on strongly, the fundamental science; its brilliant successes had been bought at the price of ignoring the infinite complexity of the real world. The justification of fundamental physics on the ground that it leads to valuable practical applications cannot withstand evaluation by even so blunt an instrument as cost-benefit analysis. These are hard-hitting, penetratingly-severe and fundamentally-important words that have received far too little attention in Australia.

(b) Physics funding

There is currently a crisis in the funding of university research in Australia, and I have already expressed some views on this topic (Jenkin, 1982).

In 1907 J.J. Thomson donated his accumulated lecturing fees (£4,000) towards the capital cost of an extension to the Cavendish, a noble and economical act which Crowther notes also consolidated the practice of running the laboratory on an excessively small budget. Rutherford occasionally agreed to large expenditures, but in general he believed that it was not necessary to have expensive apparatus to make important discoveries. His own incredible facility with simple apparatus is legendary, as are his remarks that “I could do research at the North Pole” and that “We’ve got no money, so we’ve got to think.” Chadwick argued with Rutherford especially about money, and thought that Rutherford had a profound distaste or inhibition in asking for it. Chadwick, Blackett and Oliphant later left the Cavendish, partly to have their “own shows”, but partly because they were fed up with the lack of money.

In Australia we have still not progressed beyond the string-and-sealing-wax mentality, except in a very few specific areas. The money available to the ARGC for physics is incredibly puny. I well remember a notable visitor to La Trobe University commending us on building so much of our own apparatus; only recently have we fully realised how quickly we are falling behind British, European and American scientists, who have been doing physics (on commercial instruments) while we have been struggling to upgrade our existing apparatus and to design and build new equipment. I am reminded often of the jealous ridicule heaped upon Harry Messel when he raised substantial amounts of money for physics at Sydney University at the time when I was a young graduate student.

(c) Mathematical physics

I have already given some attention to the Cavendish preference for mechanical models and mental pictures. Thomson “abhorred the worship of mathematics as the end-all of science”, and, although “he did not specify the persons whose work exemplified the super-analytic approach, it would be expected that he, like Maxwell, looked upon this regrettable trend as originating on the Continent” . . . “In Britain, there was not a single supporter of the quantum theory . . . until about 1912” (McCormmach, op. cit.).

Rutherford had a deep respect and affection for those who contributed, in the theoretical field, to the advancement of physics; but he too was suspicious of theoreticians nevertheless. His utterance are well recorded and remembered: Following a comment by Wien in 1910 that “No Anglo-Saxon can understand relativity”, Rutherford’s rejoinder “No, they have too much sense”; or, of theorists, “They play games with their symbols, but we in the Cavendish turn out the real solid facts of Nature”; or, in proposing a vote of thanks to Heisenberg for a lecture, “We are all much obliged for your exposition of a lot of interesting nonsense, which is most suggestive”.

Crowther has commented that, following the Cavendish tradition, the theoretical education of experimentalists in Britain took a rather haphazard course, whereas in the United States a much higher level of theoretical instruction for experimental physicists has developed. And in Continental Europe it had been different for many years. One only has to think for a moment of the developments of quantum theory and wave mechanics to realize that there was something fundamentally different in German society, German education and the way they viewed the world. In Edinburgh Max Born was relegated to two dark and gloomy rooms in an underground basement of the Physics building, and Rudolf Peierls to a wooden hut in Birmingham.

In Australia the record is an even sadder one. Courtney Mohr was treated shabbily at Melbourne University, and I am reliably informed that the proposal to establish a Chair of Mathematical Physics at Adelaide was opposed by important physicists, although the support of Leonard Huxley (an engineer by training) was ultimately successful. The arrival of Herbert Green to fill that Chair has been one of the highlights of theoretical physics in this country; and when Harry Messel, who was the first lecturer there, was appointed to Sydney, he brought in people like Stuart Butler and John Blatt, who certainly helped to establish a tradition of theoretical physics in Australia. But this vital area remains undervalued and under-supported throughout the country. Professor Green tells me that his department in Adelaide is at present under an implicit threat that, when he retires at the end of 1985, he will be replaced by an untenured lecturer, and there are similar situations elsewhere. There can surely be no clearer example than this of the place given to mathematical physics in Australia. And all of this in the face of Oliphant’s recent statement that “I came to appreciate the fact that mathematics applied to science was probably the highest form of human thinking” (Oliphant, 1979).

(d) Other disciplines

One of the problems of physics, it seems to me, has been the arrogance of its practitioners. Physics was not just worth studying, it was the best and often the only thing worth studying. It was Rutherford who said “all science is either physics or stamp collecting”, and it is often implied that it only needs another advance in physics to allow the deduction from first principles of the facts and laws of the lesser sciences like chemistry and biology. Lawrence Bragg, Rutherford’s successor at the Cavendish, endured sharp and persistent criticism from British physicists, who hardly recognised crystallography, and certainly not molecular biology, as physics. The Nobel awards to Perutz and Kendrew and to Crick and Watson comforted Bragg in his later years. Pippard, in his Inaugural Lecture, struck at this arrogance and complacency. Chemistry was not a mere branch of physics, he suggested, and it is a delusion to believe that the application of physical methods to biological systems will enable living processes to be reduced to the rule of physical law. Anthropology, he ventured, has more to say about the world in which we
find ourselves, for that world is more than an assembly of particles; it consists of an assembly of men and women, of thinking and feeling beings. If physicists are to keep faith with their great heritage, they should take a cool look at the claims of their predecessors.

Conclusion
I have been deliberately critical of Australian physics in this paper, for which I offer no apology and for which I expect no mercy; and I have tried to suggest some of the origins and causes of our malaise. It seems to me that we need a very vigorous and a deeply considered re-evaluation of physics in this country, and that we need it now.

REFERENCES