Man and occasion met when the Royal Society chose Captain James Cook to command Endeavour on the expedition to Tahiti in 1769 to observe the transit of Venus, a phenomenon of outstanding scientific importance. Its importance was matched by the work of Cook and his fellow-scientists on this and subsequent voyages.

Cook was a formidable man: powerful, meticulously painstaking, accurate, and patient. He was the supreme navigator of the eighteenth century, and his observations have been as valuable as they were diverse: from control of scurvy to determining the solar parallax, calculating lunar distances, and disproving the theory of a vast southern continent.

The considerable legacy of scientific accomplishments his voyages produced were the subjects of the 1969 Symposium sponsored by the Australian Academy of Science to commemorate Captain Cook's work in the Pacific. Six distinguished scientists and historians delivered addresses on Cook and his scientific companions, the observations at Tahiti, his work as scientist and navigator, the botany of the South Pacific region, and the Great Barrier Reef (on which Cook nearly came to grief).

This book significantly expands our knowledge of Cook. His voyages and achievements will be as stimulating to those of inquiring mind as they have been challenging to scientists from his own day to ours.

Price in Australia

$5.00
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CAPTAIN COOK
NAVIGATOR AND SCIENTIST
Captain James Cook, R.N., F.R.S., from the painting by John Webber (by courtesy of the National Art Gallery, Wellington)
CAPTAIN COOK

Navigator and Scientist

Papers presented at the Cook Bicentenary Symposium
Australian Academy of Science, Canberra
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EDITED BY G. M. BADGER

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INTRODUCTION

Sir Macfarlane Burnet
President of the Australian Academy of Science

It is my privilege to introduce one of the most interesting and noteworthy of all the symposia that have been associated with an annual general meeting of the Australian Academy of Science.

The symposium commemorates the bicentenary of the voyages of discovery, and the scientific work, of Captain James Cook, R.N., Fellow of the Royal Society and Copley medallist. At the same time it celebrates the decision which in due course brought this country to the knowledge of Europe, and rather directly determined its future history.

The Australian Academy of Science can claim to be in apostolic succession from, as well as being closely based in structure and function on, the Royal Society. So, as President of the Academy, I offer a warm welcome to the President of the Royal Society. There is a special appropriateness in the fact that Lord Blackett—once Lieutenant Blackett, R.N.—President of the Royal Society and Copley medallist, should be the first contributor to the symposium. The Academy is equally happy to have Professor J. C. Beaglehole, the foremost authority on Cook’s achievement, and to welcome back one of its own Fellows, Sir Richard Woolley, the Astronomer Royal.

I suppose that everyone has his own special choice amongst the great men of the past, and I suspect that such a choice tells more about the chooser than the chosen. My own, for what it is worth, is Erasmus of Rotterdam, Cook of Whitby, and Darwin of Down House.

Cook was a remarkable man. His reputation rests almost entirely on his accomplishments as a naval officer in peace-time. He fought in no great naval battles, but is chiefly remembered for his qualities of leadership, for his exploits as an explorer, and for his skill as a navigator and scientist. As the inscription on the monument erected to his memory by Sir Hugh Palliser testifies, he ‘raised himself solely by his merit, From a very obscure birth to the rank Of Post-captain in the Royal Navy’. ‘He possessed,’ it continues, ‘in an eminent degree, all the Qualifications requisite for his profession And his great undertakings; together with the Amiable and worthy qualities of the best of men. . . .’
Cook went to sea as a boy, and he entered the Royal Navy as a young man. His obvious qualities, his intelligence and perseverance, led to his appointment as Master in command of a schooner engaged on surveying work. On 25 May 1768 he was commissioned as Lieutenant and given the command of the *Endeavour*, destined for the South Seas to observe the transit of Venus for the Royal Society and to make geographical discoveries. He arrived at Tahiti on 13 April 1769, in good time to set up the instruments to observe the transit on 3 June. Then in 1770 he discovered and charted the east coast of Australia.

The success of the first voyage persuaded the Admiralty to give Cook the command of a second expedition to the Pacific; and a third voyage was also undertaken. These voyages were all notable for their contributions to knowledge of the islands and of the countries of the Pacific. In this realm of discovery Cook’s own contributions were augmented by those of the several ‘scientific gentlemen’ who accompanied him: Green, Banks, and Solander on the first voyage; the Forsters, Bayly, and Wales on the second; and Bayly on the third.

For a special reason which will emerge in due course, I want to comment on Cook’s companion on his first voyage, Joseph Banks.

In 1768, when the voyage commenced, Banks was a very wealthy young man of twenty-five with a taste for adventure and natural history. Botany was his main academic interest, and he was largely responsible for the development of Kew Gardens and their scientific activities. From our point of view, however, his most important subsequent activities were in regard to the colonisation of Australia and his forty-two years as President of the Royal Society (1778-1820), from the age of thirty-five to that of seventy-seven. Banks was probably one of the most important non-political figures in the England of his day, and played a major part in increasing the status and influence of the Royal Society. It is pleasant to read that Banks valued the Presidency of the Society above any other distinction he received . . . . He made it his regular practice to preside at its meetings in Court Dress and wearing his decoration so as to honour suitably the institution whose patron was the sovereign.

His role in the foundation of settlement in Australia appears to have been crucial. The first suggestion that Australia would be the best substitute for the lost American colonies as an overseas penal settlement came from Banks before a committee of the House of Commons in 1779. He believed that Australia was ideally suited for the purpose—so distant from civilisation that escape would be difficult; the natives could offer no opposition; the climate was mild; there were no beasts of prey, and there was plenty of firewood!
Fig. 1. Sir Joseph Banks, President of the Royal Society, from an engraving of the painting by T. Lawrence (from the Rex Nan Kivell Collection, by courtesy of the National Library of Australia, Canberra)
Fig. 2. Painting by Celia Rosser of *Banksia serrata*, presented to the Royal Society by the Australian Academy of Science
INTRODUCTION

So we had the first fleet and the convicts and emancipists; and the words 'Botany Bay' and 'Australia' developed a connotation in England that they took a very long time to lose. But Botany Bay had been so named because it offered a veritable paradise of new discoveries to Banks and Solander in 1770. Amongst their findings were our coastal honeysuckle trees with their fuzzy flower heads and woody seed cases. There are many trees and shrubs of their genus, but the common one at Botany Bay became the type of the genus created by Carl von Linné jun., son of the great Linnaeus, and named in Banks's honour, *Banksia serrata* (see Fig. 2).

It seemed to the Council of the Academy that it would be an appropriate gesture on this occasion to commission for presentation to the Royal Society a painting, which I believe is in the best convention of classical botanical work, of *Banksia serrata*.

EDITOR'S NOTE

Lord Blackett accepted the painting on behalf of the Royal Society, and then asked Sir Macfarlane Burnet to accept, on behalf of the Academy, a volume containing the

Directions to be observed by Captain James Cook and Mr. Charles Green, with respect to their making astronomical observations in the Pacific Ocean and in the voyage and home again being a copy taken from the Minutes of the meeting of the Council of the Royal Society of London held on 23 June 1768.
Captain Cook has long been one of my heroes, as one of the greatest navigators and explorers of history. In addition, he was, in Fanny Burney's words, '... the most moderate, humane and gentle circumnavigator who ever went upon discoveries'.

Cook's first voyage, which we celebrate 200 years later, occurred at a turning point of the technique of navigation. His navigation on his three years' voyage depended for longitude on the measurement of the angular distance of the moon from the fixed stars. It could take up to four hours of numerical calculation to work out the longitude. Cook had clearly trained himself to be a first-rate observer and an accurate calculator. This method of lunar distance became of practical use at sea only when the essential astronomical data became easily available to seamen. This happened in 1767, the year before his voyage, because of the publication of the \textit{Nautical Almanac and Astronomical Ephemeris} by the Astronomer Royal. By this method Cook was generally able to determine the longitude to less than thirty miles.

However, by the start of Cook's second voyage in 1772 a reasonably accurate chronometer, made by Kendall but deriving from Harrison's pioneer work, had become available. Though not as reliable a time-keeper as was desirable, it was still a great improvement over lunar sights alone for the determination of longitude at sea.

Essentially no major change in the technical methods of navigation at sea appeared till this century. When, over fifty years ago, I was taught to navigate by taking stellar and solar sights in the South Atlantic, the methods in use were the same as in Cook’s day 150 years earlier, except for the radio time signal, which had then only just begun to come into use. The sextant used was identical in design to the Ramsden instrument of the 1770s. Though, of course, the radio time signals eventually greatly reduced the demand on the performance of the chronometers, still, in my youth at sea, the winding and care of the
ship's chronometers was carried out daily with regularity and solemnity—as no doubt it was in Cook's ships.

Without accurate chronometers or radio time signals, as Cook was on his first voyage, the lunar distance method was the only way of finding one's longitude at sea. So, though now only a memory, navigation by lunar observations played a vital role, not only in the history of navigation, but in the history of science itself. For the problem of finding the longitude at sea was the dominating concern of the rising mercantile powers in the sixteenth and seventeenth centuries, and resulted in the active concern of governments with astronomy. In Britain this concern led to the founding of the Royal Observatory in 1675 by Charles II: four years earlier the Royal Observatory of Paris had been founded. The longitude problem also led Newton to devote much personal effort to the improvement of lunar tables. For many years he presided over the Bureau of Longitude.

From its beginnings in 1660, the Royal Society had been closely concerned with the advance of astronomy. In the eighteenth century the measurement of the time it took for Venus to transit the Sun's disc was the best method of determining the distance of the Earth from the Sun—and so the whole scale of the solar system. In 1766 the Council of the Royal Society made a preliminary study of the problem of observing the transit of Venus which would take place in 1769. In February 1768 the Royal Society sent a memorial to King George III for financial support. The King almost immediately granted the sum of £4,000 for a voyage to the South Seas. So was launched one of the first major scientific voyages planned for a clearly defined scientific objective. Although the general advance of astronomy was then recognised as an important national goal because of its importance for navigation, there were other reasons for the alacrity and generosity of the King's response to the request from the Royal Society. Undoubtedly Cook received secret instructions from the Admiralty to do as much general exploration as could be fitted into his astronomical program; in particular to discover whether there were any new inhabited lands to trade with, and in general to forestall our then main colonial rivals, the French. The foundation of Australia as a colony by the British rather than the French was an indirect result of Cook's voyages.

The Royal Society threw itself with great vigour into the task of organising the expedition. The Astronomer Royal was asked to supervise the provision of the latest scientific and nautical instruments by the leading instrument makers of the day: telescopes, quadrants, sextants, etc. An elegant, tall, pendulum clock by Shelton, which was either the one used by Cook during the astronomical observations on
Fig. 3. Pendulum clock made by John Shelton. This is either the one used by Cook during the astronomical observations on shore at Tahiti, or an identical one, and stands now in the office of the President of the Royal Society (by courtesy of the Royal Society, London).
shore at Tahiti, or an identical one, stands now in my office in Carlton House Terrace. The great engineer Smeaton provided a portable observatory to house the instruments when on shore.

Paramount, of course, was the purchase of the *Endeavour* by the Admiralty. The story of how a relatively unknown naval officer, James Cook, forty years of age, was chosen to be Captain of the *Endeavour*, of her fitting out, the appointment and characters of the scientists (Banks, Solander, Green, Parkinson), of the selection of Tahiti as the island from which to make the observations—all this has often been told; much of it is found in those most readable of books, *The Fatal Impact*, by Alan Moorehead, and *Captain Cook*, written by Alan Villiers. The January 1968 number of the journal *Endeavour* has valuable articles on many aspects of Cook's three voyages.

Cook himself may be said to have foreseen more clearly than most of his contemporaries the future of these island communities when he wrote, after seeing the effects of venereal disease and firearms and other Western introductions, that it would probably have been better for the islanders if they had been left alone by the Europeans. He seems to have had an intuitive insight into the minds of these peoples, and, as it has been said, they worshipped him after a fashion.

A subsidiary achievement of Cook relates to his second voyage, lasting three years, when he lost only one man by disease out of over a hundred. How he did it is described by him in a short paper in the *Philosophical Transactions of the Royal Society* of 1776, entitled 'The Method taken for preserving the Health of the Crew of His Majesty's Ship the *Resolution* during her late Voyage round the World'. In the same year Cook was elected a Fellow of the Royal Society, and also granted its highest award, the Copley Medal.

Though secondary to the main objectives of the expedition, the botanical work of Banks and Solander in collecting and describing 1,300 new species of plants comprising 110 new genera resulted in large measure from the indefatigable enthusiasm of these two men. Banks, the wealthy and cultivated land-owner, was an amateur of exceptional gifts, interested in everything—good qualities perhaps for his unique record of later holding the Presidency of the Royal Society for forty-two years.

James Cook, son of a Yorkshire agricultural labourer, who left his village school at the age of twelve and learnt his seamanship in the North Sea coal trade, in the Gulf of St Lawrence, and on the coasts of Newfoundland, was the second in the line of the three greatest seamen of British history—Drake, Cook, and Nelson. Drake was outstanding as a buccaneer and explorer; Nelson as a supreme commander in naval warfare; Cook as a genius in scientific navigation and exploration.
I have been thinking about Cook for something over twenty years; and
one of the aspects of Cook I suppose I have been thinking about is
Cook the Man. I don’t know quite what you, or I, mean by the phrase.
Do we mean Cook the private man, as distinct from a professional or
public man? I know what Captain King said about him, and what
Mr Surgeon Samwell said about him, and what J. R. Forster said, and
William Bligh, by implication, said, and what some of the midshipmen
said; and some of what they said is useful, and some—I won’t say is
useless, for it throws some light on them—does not throw much light
on Cook. I presume we want to get ‘inside’ Cook, we want to pierce to
some secret spring. We light with pleasure on some odd bit of informa-
tion, such as that he did not like bananas. I have seen this stated as a
fact, and all at once it seems illuminating—though I am bound to say
I have found no support for the assertion in any of his own words.
I deduce myself from some of his own words that he did not like
pumpkin, and I confess that when I made the deduction I was greatly
pleased: here, I thought, at last, is a ray of light. But is it a ray of light
or merely a piece of amusement? The centre, the core, the truth, the
essence, the innermost man, for the American movie moguls—as I
have reason to know—would lie in the revelation of some desperate
liaison with a Tahitian or Hawaiian or any other Polynesian princess;
and of course, as there appears to be no such thing, the movie moguls
have lost all interest in celebrating him. But here I ask the question,
would hypothetical, or even secure, knowledge of this sort of thing—
his attitude to bananas or pumpkin or the South Sea Fair—bring us
nearer Cook the man than our knowledge of Cook the navigator or
Cook the scientific observer? If there is one phrase that comes in like a
refrain in his journals, it is the phrase, ‘Geography and Navigation’.
Then we might argue that Professor Badger and Sir Frederick White
will tell you far more about Cook the man than I can. In the end that
is the man—the sailor, the geographer, what I shall call the passionate
professional.

You are not satisfied, and I am not satisfied, because, as human
beings, we want not merely to be stunned by a statue, but to peer into a human being. We want a man with human attachments, failings, agonies, we want our heroes to be sometimes wrong, we recoil from excessive virtue. I myself would give the world for a letter to Mrs Cook—though I am sure that would not pass the bounds of virtue. I cannot describe my enthusiasm when I found out that the Captain swore—the pumpkins paled into insignificance. Now I think that if we read everything that Cook wrote, journals, drafts, letters, and read between the lines, and read what his men and his associates wrote, and sometimes between their lines; and if we consider all this intently enough, with carefully controlled imagination, in the context of eighteenth-century social and political, scientific and naval history—we shall stand some chance of getting inside Cook: more chance than most people, including myself, have thought in the past. Unfortunately I cannot now dictate a book to you, or quote, as I should, for two or three hours from himself. Let us take the history of the voyages for granted, though always in the back of our minds; let us even take for granted what I suspect is for Australians the principal dispensation of Providence in the eighteenth century, acting through one man—the discovery of what Cook called New South Wales.

We had better start with the outside of the man, however, not his inside: or at least with the physical man. Putting our informants together, and the portraits, we get a big man, something over six feet, large-boned, powerful, with strongly-marked features. I don't know what his parents were like—you will remember he had a Yorkshire mother and a Scottish father—but I think he must have been one of the Yorkshire types. I have seen Yorkshiremen, both academic and manual labourers, with contours, as it were, almost startlingly like his. He was good-looking in a plain sort of way. We have one contradiction in description: Samwell said his head was small, with small eyes, but the portrait by Nathaniel Dance, that we all know, said by Samwell to be 'a most excellent likeness', gives us a large rather than a small head, and large eyes that match the large nose, wide mouth, large forehead. Webber also gives us a large head—and a heavy, dull one. Eyes brown, 'quick and piercing', prominent eyebrows; dark brown hair tied behind, though in the Dance portrait, and the full length by Webber, he seems to be wearing a dark grey wig. His face was full of expression, says Samwell: well, yes, it looks as if it might be the face of a man with the most friendly, benevolent, and humane disposition. I use the Samwellian adjectives. But obviously it could look quite other; and that large frame stamping round the deck in a rage, those strongly-marked features and brows, those piercing eyes, denouncing incompetence or stupidity or dirt, must have been a very terrifying
spectacle indeed for the sinner. What sort of voice? No one says. Presumably it could be loud: it had to compete with a good many storms, as well as with human misdemeanours. On the other hand there was the agreeable lively conversation, sensible and intelligent, there was the modesty; Samwell even says he was a rather bashful man, which I take to imply modesty again, and sensitivity, not awkwardness; and these characteristics do not argue noise. I should infer a provincial accent, some provincial turns of speech, and some provincial pronunciations, to match some of the spellings in his journals. On the physical, the visible side, then, perhaps on the audible side, a man with an original endowment of rude and plentiful good health, a country boy of strong ancestry and open-air upbringing, hard but not crippling farm work, hard work as a young seaman but with the watchful and benevolent eye of Quaker John Walker on him, so that when he came into the navy in his late twenties he had the stamina to withstand the hardships of that dreadful and noisome service. And notice that the physical endowment went with a matching endowment of mind, which was never laid to rest: in the coal-shipping trade crews were small enough to put some responsibility on every man; in the navy he was almost at once given the minor responsibility of a master's mate, and in two years the major responsibility—the responsibility for a ship—of a master; in a few years more he had the responsibility of independent command and independent scientific work as a surveyor of Newfoundland. So far as I know, he had no serious illness till the age of forty-five; and behind that illness, I should say, lay a remorseless mental as well as physical strain.

Now that we have got round to this side of him, we can dip in among the terms used by his admiring observers, and come up with much the same ones, whoever the observers are. Cool, courageous, firm, vigilant, active, resolved, humane, patient—both passionate and patient—unaffected, of unremitting perseverance—and I have used some of the others already. I don’t think we can deny any of this, even if we fear to become implicated with perfection. But we have no St James of Marton-in-Cleveland, or Botany Bay, or Latitude 71° 10' S to deal with. Searching around, as I do, for the man—some leading characteristic, some not very secret spring—I light on what I shall call not perseverance or resolution or even determination, but stubbornness: stubbornness because I see some sort of element of native aboriginal rock in his make-up, by the side of which the other words seem almost tinged with a self-conscious intellectualism. At the same time I won’t go so far as to say obstinacy, which implies a bit of the mulish, or pig-like, and Cook stopped before he reached that point. I can illustrate this, first of all and conveniently enough, by recurring to the bananas,
Fig. 4. Cook's chart of Tahiti, showing Matavai Bay and Point Venus (reproduced from Hawkesworth)
or the pumpkin. I did not start with them for the purpose of being facetious at any cost. If you will go to Captain King, you will find that he almost begins his sketch of his hero by adverting to the same subject:

His stomach bore, without difficulty, the coarsest and most ungrateful food. Indeed, temperance for him was scarcely a virtue; so great was the indifference with which he submitted to every kind of self-denial.

At this Samwell protested. Of course temperance was as great a virtue in him as in any other man. ‘He had no repugnance to good living; he always kept a good table, though he could bear the reverse without murmuring.’ In the Arctic, in 1778, they were killing walruses for food. There were few on board, said Cook, who did not prefer walrus to salt meat. ‘Captain Cook here speaks entirely from his own taste’, says Midshipman Trevenen, ‘which was, surely, the coarsest that ever mortal was endued with.’ Near the Endeavour River, in 1770, wild taro was discovered: ‘the tops we found made good greens and eat exceeding well when boil’d [it is Cook speaking] but the roots were so acrid that few besides myself could eat them’. Yet this was the man who appreciated so highly Tahitian-cooked dog: ‘few were there of us but what allowe’d that a South Sea Dog was next to an English lamb’; discriminated against the ‘sour and disagreeable’ preserved bread-fruit; was devoted to the pudding called ‘poe’—‘I seldom or never dined without one when I could get it’; and wrote down recipes. The same man would eat, outside ship’s rations, anything he could get hold of in the way of vegetables and fruit, fish, flesh, and fowl, not just walrus but seal—a seal steak was excellent and a sea-lion cub was very palatable; not just New Zealand ducks and Tierra del Fuegan geese but anything in the way of a sea-bird—he was very partial to a young shag. He could not say that penguins were good eating: ‘I have indeed made several good meals of them but it was for want of better victuals’; they were, at any rate, preferable to the salt beef and salt pork that after three years came out of the harness-cask. After seeing severe fish-poisoning in the New Hebrides he insisted on trying another fish that he was warned was poisonous in New Caledonia, asserting that he had eaten it quite safely on the coast of New Holland, and was accordingly badly poisoned himself; and there is stubbornness for you. On the whole I am inclined to accept Samwell’s judgment, not King’s or Trevenen’s. Why? Well, you will recollect that scurvy-grass and wild celery, seals, walrus, and penguins, palatable or not, were all fresh food. You will recollect what Cook was fighting against. He was fighting against that ancient terror of the sea, so much worse than winds and waves, the disease of scurvy; and he had a fighting faith in fresh
food. He was responsible for the health of a hundred—more than a hundred—men. As much as any great experimenter in the human cause, he would take the lead himself. He did not want his men to go down with scurvy, and he was certainly not going down with scurvy himself.

And he knew his men; he knew the stubbornly conservative British sailor. He was a practical psychologist as well as a practical dietitian, and their stubbornness was no match for his. Well, most of the time. It was only early in his first voyage that he flogged men for refusing their ration of fresh meat. Thereafter he trusted to the force of example, to the natural wish of mankind not to be excluded from any imagined good, and to the removal of any alternative. By the time he had reached Tahiti on his first voyage he was writing his famous reflection on seamen and novelty in diet:

such are the Tempers and dispositions of Seamen in general that whatever you give them out of the common way, altho it be ever so much for their good yet it will not go down with them and you will hear nothing but murmurings against the Man that first invented it; but the Moment they see their Superiors set a Value upon it, it becomes the finest stuff in the World and the inventor a damn'd honest fellow.8

The amount of green vegetable—wild celery and so on—he got into his men by cooking it with their breakfast wheat or pease in remote parts was enormous. And the honest fellows were trained in the habit of looking for it as they wandered on shore. They did so: they learned soon enough that there was a sure way into the Captain's favour. There were stiff-necked fellows, of course, even on the third voyage, who were tempted to curse.9 All right, as long as you didn't curse the captain to his face. There were unfortunate fellows, too, on this third voyage, who could not make away with their walrus, though the majority were ravenous enough at first. Very well, said Cook, they can eat their bread—that is, the appalling ship's biscuit; and only after much persuasion would he admit that fluxes and sickness were the results not of malingering but of genuine rejection by the stomach, and give back the equally appalling salt meat. He had one or two struggles over spruce beer, brewed from the tender branch-ends of trees like the New Zealand rimu, on the model of a drink popular among North American sailors and fishermen, and won. He lost over sugar-cane beer on the Hawaiian coast in December 1778, and even the example of himself and his officers could not prevail. The episode is interesting, because we find Cook, most unusually in his journal, both denouncing his crew—'my mutinous turbulent crew'—and praising himself. Cook thought the brew was very wholesome. The crew thought it injurious to
their health, and told him so in a letter. He told them the letter was a very mutinous proceeding, stopped their grog, and declared that in future they might not expect the least indulgence from him. He almost used the words of his first journal, but went on with a difference:

Every innovation whatever tho ever so much to their advantage is sure to meet with the highest disapprobation from Seamen, Portable Soup and Sour Krout were at first both condemned by them as stuff not fit for human beings to eat. Few men have introduced into their Ships more novelties in the way of victuals and drink than I have done; indeed few men have had the same opportunity or been driven to the same necessity. It has however in a great measure been owing to such little innovations that I have always kept my people generally speaking free from that dreadful distemper the Scurvy.\textsuperscript{10}

I think if he had lived to revise thoroughly this part of his journal he would have deleted the whole story, as he deleted any other reference that seemed to discredit his people. And the conflict seems to have faded away. The real trouble may have been that everybody was becoming rather edgy: they all, except Cook, desperately wanted to get into port, after their arctic months, and he—stubbornly again—was keeping them out of port, to avoid too great a rush on the buying of provisions, and consequent waste; and also, so far as he could avoid it, too great a rush on the Hawaiian women. Now I reiterate my point about all this. Cook did not invent fresh food: he was not a damn’d honest fellow to that extent. Medical men with a mission, Dr James Lind, Dr William MacBride, harped on it. Sea captains in general knew about it, and got it when they could, and thought about it. The trouble was that they did not think about it enough. Cook’s second in command on his second voyage, Tobias Furneaux, a very competent sailor, is a beautiful example. But only stubbornness in pursuit of his principle would have brought Cook back to England at the end of that second voyage, after an absence of three years and eighteen days, in which time—I quote him again—he ‘lost but four men and only one of them by sickness’.\textsuperscript{11}

Of course, remarkable as Cook’s achievement was in the preservation of health at sea, and important as this was for his work as an explorer, the Admiralty did not send him out primarily to conduct experiments on diet. He was sent out on his first voyage primarily for the observation of the transit of Venus, and then to see if he could pick up the alleged southern continent, or if that failed, New Zealand. He picked up the east coast of New Holland—Australia—as a work of supererogation, a bit of extra thrown in, a bonus for the Admiralty. No one expected
him to, or suggested it. He was sent out on his second voyage, on his own suggestion and his own plan, to clean up the continent problem. He was sent out on his third voyage to look for the Northwest Passage. In other words, his principal, his over-riding, his sovereign purpose was, to use the phrase he used so much himself with capital letters, Geography and Navigation. This may savour to you all too much of the public Cook, the statue in the Admiralty precincts or the Sydney Domain; but we quite mistake him if we do not realise that this, in the innermost sense, was Cook the man. Why the Yorkshire farm boy turned into that passionate professional I do not know. How he did so, at least to the outside view, is fairly plain. If we burrow inside as far as we can, we see this quality of stubbornness again. We can go back behind the great voyages to his first independent command, the schooner Grenville, surveying the Newfoundland coast. She was hardly on station in the 1764 season when a powder-horn burst in Cook’s right hand and almost blew his thumb off. We know what would happen in these days: the radio would crackle away, a helicopter would be out, and in an hour or two the man would be in hospital and under anaesthetic. The Grenville got into the nearest harbour and by great good luck found a French ship with a surgeon on board; the hand was properly dressed, according to the notions of the time; and Mr Cook went back to his surveying and produced a beautiful chart. It was the professional attitude. It is what makes Cook’s predecessors of the 1760s, Byron, Wallis, Bougainville, look such hopeless amateurs; yet Bougainville had the highest intentions, and Wallis looked after his men like a father. Only Carteret had that quality of stubbornness, and his reward from his masters was to be put on the shelf. Yet even Carteret had not the professional equipment. Cook did. He was the only man in the navy capable of carrying out his instructions, though the technical ones were repeated from those given to his predecessors, and probably the only man in the navy with the inclination to obey his instructions. I have sometimes wondered whether there was a sort of literalism in Cook, or whether his essence was due partly to the fact that he sprang from the respectable lower classes, whose function in life was to obey, and not from that upper class who gave instructions, and therefore knew what they were really supposed to mean; but this may be fanciful. Anyhow, let me illustrate my thesis.

Let us begin with the first voyage. It is 14 December 1769, and Cook is off the North Cape of New Zealand, in sight of it, in a hard gale for the previous thirty-six hours, with a heavy swell from the west; the gale keeps on and his canvas starts to go. He is driven out of sight of land, east, northwest, but he wants to get back and fix the position of that cape. It takes him four days. He is not standing on the cape, but
is four or five miles off as far as he can judge, in poor conditions; he
has no chronometer; and he is four minutes out in latitude and one
minute out in longitude. He is driven off the land again, about 140
miles to the north. Then he has a hurricane, and twice has to bring
the ship to; she is blown, to put it briefly, all over the place. But he is
going to get the position of Cape Maria van Diemen, only a few miles
from the North Cape; he sees it at last on 30 December and fixes its
position, the weather having cleared, three days later, two minutes of
latitude out, four of longitude. He says, moderately,

I cannot help thinking but what will appear a little strange that at
this season of the Year we should be three weeks in getting 10 leagues
to the westward and five weeks in getting 50 leagues, for so long it is
since we pass’d Cape Brett but it will hardly be credited that in the
midst of summer and in the Latitude of 35° such a gale of wind as we
have had could have happen’d, which for its strength and continuence
was such as I hardly was ever in before. Fortunately at this time we
were at a good distance from land, otherwise it might have proved
fatal to us.12

The amateur would not have clung on like that. But it was part of
Cook’s job to fix positions. I need hardly elaborate to an Australian
gathering the stubborn persistence of his charting of the Australian
coast, and the way in which, having got free of the Great Barrier Reef,
he stayed by it through one of the most hair-raising episodes in the
whole history of exploration, because he wanted to verify the existence
of a strait between Australia and New Guinea. Let us pass to the
second voyage, which was an icy one, wherein he laid the foundations
of antarctic oceanography. He knew nothing about ice navigation for
a start, though his ship had been stuck once for a few hours off the
Nova Scotian coast in the spring of 1759. The combination of bergs,
pack-ice, either in the mass or in disintegration, gales, fog, sleet, and
snow should have been too much to bear, either for the body or the
mind—particularly the mind of a commander. There is really only
one way to come near appreciating this achievement, and that is to
read it in detail, carefully and with some imagination, in Cook’s
own journal and the journals of his men, all of whom had not his talent
for understatement; and to remember that when he uses words like
horrid, horribleness, frigidness, savage, disagreeable anxiety, he is
writing with no romantic pen. If he says the ship was in danger we may
conclude that she was in considerable danger. He was the first man to
cross the Antarctic Circle, and he crossed it three times, and where he
reached his farthest south no ship has ever been since; and, having set
out to circumnavigate the world from west to east in the highest possible
latitude, he did so, though often enough he found himself driven west instead of east, as he became acquainted with the antarctic winds, or had to turn north through sheer necessity of extrication. As we know, he shattered the southern continent theory to bits, so far as it was a theory of a habitable land (nobody could foresee present-day goings on), though he believed that a continent of some sort did exist, and that the South Sandwich Islands, viewed fragmentarily through the fog, and 'the most horrible coast in the World', might be its northernmost extension; if anybody had resolution and perseverance to clear up the point by going farther than he had done, 'I shall not envy him the honour of the discovery but I will be bold to say that the world will not be benefited by it'.

The second voyage gives us a further illustration. Cook had determined that the months of antarctic winter should be spent not in lying up in harbour—the very idea is violently alien to his character—but in two sweeps about the eastern and central island groups of the Pacific to make what new discoveries he could and co-ordinate the old ones. The second and greater of these sweeps was introduced by Cook's extremely serious illness at the end of February 1774, which would have been a good enough excuse to go home. He went to Easter Island, and there began a sort of parenthesis in his major theme, which in itself would have made a brilliant reputation for any other man. (Why, by the way, do we never apply the adjective 'brilliant' to Cook? Perhaps because he was so much more than brilliant.) I shall not particularise this parenthesis, I shall merely advert to his tracing of the northeastern and southern coasts of New Caledonia in September of that year. He spent a week in harbour, and made one additional brief landing; for almost the whole of the rest of that month he was at sea, on a lee shore, outside a barrier reef, in winds that had an inconvenient habit of dropping at critical times.

Friday 16th. At 3 pm it fell Calm and we were left to the Mercy of a great swell which set directly upon the reef which was hardly one league from us, we Sounded but could find no ground with a line of 200 fathoms.

What does that remind you of? Of the Australian Great Barrier Reef in August 1770? He had the boats out to tow, 'but they were of little use against so large a swell'. The echo comes only too clearly. He charted that coast; he got round the Isle of Pines into a cul-de-sac of reefs and islets, and spent a night running from one set of breakers to another that almost turns a man's hair grey to think of it; and what did he do next morning? 'I was now almost tired of a Coast I could no longer explore but at the risk of loosing the ship and ruining the whole
Voyage'; so as a preliminary to leaving it he ran further in—because he wanted to look more closely at that extraordinary tree, the *Araucaria columnaris*. Was he rash? No, he knew what he was about. But I think he was stubborn.

From the third voyage I take only one example, lest I weary you, his fight to make easting, against cruelly contrary winds, after he left New Zealand for Tahiti in March 1777; and I take that example because he was beaten, and lost a season, though he gained consequentially much information about Tonga and Tahiti that we are very glad to have.

I do not wish to convey an impression of an automatic man of iron. Can anyone doubt that, after the escape from utter destruction outside the Barrier Reef in August 1770, as he sat down to write at his cabin table, he had some anguish of reaction, with his head on his arms? Two days before, his utmost wishes had been crowned by getting clear of the shoals inside it; now he was happy to get back among them.

Such are the vicissitudes attending this kind of service and must always attend an unknown Navigation: Was it not from the pleasure which naturally results to a Man from being the first discoverer, even was it nothing more than Sands and Shoals, this service would be insupportable especially in far distant parts, like this, short of Provisions and almost every other necessary.15

And he proceeds to reflect rather bitterly on the world, on society: if you leave off your work because of its danger you are accused of 'Timorousness and want of Perseverance'; if you carry on too long, of 'Tenuity and want of conduct'. Perhaps he had been imprudent, but at least he had found out something; and he turned to the shoals again. You may remember his words when he was brought up by what seemed illimitable ice in latitude 71° 10', the permanent ice-shelf, at the end of January 1774, the words on his own ambition: 'I whose ambition leads me not only farther than any other man has been before me, but as far as I think it possible for man to go...'. What follows immediately after? A confession of his discouragement: he 'was not sorry at meeting with this interruption, as it in some measure relieved us from the dangers and hardships, inseparable with the Navigation of the Southern Polar regions'.16 If we dislike to admit that our hero is ever discouraged, we shall find no comfort in his remark a year later, as, having struck five degrees south from his Isle of Georgia, he altered course east, with none but penguins, snow petrels, and whales to witness him: 'besides I was now tired of these high Southern Latitudes where nothing was to be found but ice and thick fogs'.17 We may note that he went on to find the South Sandwich Islands before he turned finally north to the
Cape. If we relunct very much at the term 'discouragement', we can, of course, call it a sense of proportion; and we can then relate it to a sentiment of which he delivers himself more than once, after having considered the possibility or importance of some minor uncertain piece of discovery—his unwillingness to spend time 'searching for what I was not sure to find'. 18

This quality of stubbornness is visible in a different direction. We have a less admirable side of Cook. It is not visible in his early life, or in his first and second voyages. It does come clearly enough to the eye on his third voyage. There is a paradox in it, because it appears just when he is beginning—I will not say, 'to lose his grip', that would be absurd: beginning, rather—to have less than an absolutely certain grip on every circumstance. The stubbornness may now indeed be a symptom of the little loss of grip, of a little loss in fertility of resource, in itself the result of a hidden physical and mental tiredness, hidden very likely by Cook even from himself. I have discussed this hypothesis elsewhere, and I shall not do so at length again. I by no means assert it as a fact. But the doctors who have considered his case seem agreed that his second voyage sickness, by no means as short as Cook indicates it was, was some kind of ulceration of the stomach, and he had worries and responsibility enough to bring that on. He had no fallow period between his second and third voyages, and the condition of his ship caused him concern almost from the day he left England in July 1776. He was haunted by the fear of missing that season; and the Pacific winds, that I have already referred to, sealed that loss. So when he arrived in Tonga he was not in a very fit state to withstand irritations. He was not irritated by being asked to strip to the waist as a condition of witnessing an important ceremony: he was perfectly prepared to do that, though one of his lieutenants thought it rather lowering. The principal irritation he suffered from was theft. He simply could not keep the nimble-fingered Polynesians from making off with the ship's property. He had had enough trouble on his previous voyages. He objected to killing men. He did not treasure the memory of having done so at Poverty Bay in New Zealand, when he was there in the Endeavour, though he had thought rather miserably that that might be justified as self-defence. When Lieutenant Gore, at Mercury Bay in New Zealand, snatched up a musket and shot dead a Maori who had cheated him of a piece of cloth, Cook was not pleased. 'We had now', he writes, 'been long enough acquainted with these People to know how to chastise trifling faults like this without taking away their lives.' 19 He had tried all sorts of ways, from kidnapping chiefs to flogging; and now here was the confounded exasperating business starting all over again. He settled for flogging, and for weeks he flogged—a dozen lashes, two
dozen, three, four, five, six dozen. When that failed, he ordered ears cropped, and had men's arms slashed cross-wise with a knife. There were those among his own men who were distressed and baffled. Was this the captain of whose humanity they had heard so much? He did not gain his end, and in Tonga there were those who remembered him without love. At Moorea, four months later, he tried a different plan, to get back a wretched stolen goat, staging a regular punitive expedition, burning houses, smashing and burning canoes, threatening not to leave a canoe on the island. This was after two days of negotiation, threats, and search; and after, not before, Cook began to feel he was making too much fuss:

I was now very sorry I had proceeded so far, as I could not retreat with any tolerable credit, and without giving encouragement to the people of the other islands we had yet to visit to rob us with impunity.

How often, in the history of the world, has the stubborn man put forward such excuses. I can't well account for Capt Cook's proceedings on this occasion; as they were so very different from his conduct in like cases in his former voyages', says Midshipman Gilbert, forced more than once to be a critic.

You may think it curious if I say after this that he was a patient man, and few people ever seem to have said it before. In temper subject to hasty and passion, says King; somewhat hasty, says Samwell, who tends to draw on King for his phrasing. They were both men whose personal knowledge was confined to the third voyage, and Midshipman Trevenen, who gives us the account of what the men called Cook's 'heivas', or Tahitian dances, his vehement outbreaks of rage and swearing at misdemeanours or acts of stupidity, is also a third-voyage witness; and we may note that with all these heivas—Trevenen came in for one himself, that turned out to be quite undeserved—and all the hard work, this Midshipman kept his Captain on a very high pedestal indeed. It is Trevenen, in fact, who gives us the picture of the Captain, the 'despot', in the midst of heavy boat-work unbending to relate to the young gentlemen anecdotes of his earlier voyages, and at the end of the day tossing to them the ducks that had been shot. There are no accounts of heivas from the first and second voyages, so far as I know, though I can think of two occasions when some people at least held that Cook acted unjustly in matters of discipline—and more general blame came from John Reinhold Forster, who thought that he was 'a cross-grained fellow who sometimes showed a mean disposition and was carried away by a hasty temper', and displayed an 'overbearing attitude which was the result of having his head turned by Lord Sandwich', the First Lord. But we must remember that John
Reinhold Forster, a cross-grained fellow himself, was not exactly an unprejudiced witness, and that he had a particular down on Lord Sandwich; and we should remember, too, his judgment that Cook's faults 'were more than counterbalanced by his superior qualities'. Still, there it is: in one period of his life, anyhow, Cook's temper blazed up very suddenly and very high, even if it subsided equally rapidly. And, says King, he showed a certain impatience in times of unavoidable rest, when he could neither act nor plan further action. Have I then created another paradox by calling him patient? I do not think so. Let us look at Samwell again: 'patient and firm under difficulties and distress' is one of his phrases. Surely patience is the other side of stubbornness, in the ice, blown off a coast, baffled day after day by head winds, and clinging on to a sovereign purpose. If you go through the journals carefully, and study his meetings with native peoples, you will find ample evidence of patience, from Dusky Sound in New Zealand to St Lawrence Bay on the Asian shore just south of Bering Strait—and not only of patience, but of tact and courage too. He could well understand that islanders should be distrustful of invaders from the sea. In spite of floggings and those astonishing acts of destruction at Moorea, there is ample evidence of patience in the process of living side by side with native peoples for weeks at a time; and patience was all the more necessary in those weeks because of the necessity of keeping British sailors, officers as well as men, from making rash and overbearing fools of themselves. Justice has to be seen to be done; and justice is founded upon patience. If another instance of patience with persons is needed, let me take the case of Lieutenant Gore. Gore was a seasoned sailor, whose first voyage with Cook was his third round the world, a good, practical seaman, though no great navigator, a sporting type who caught the first sting-ray and shot the first kangaroo, unromantic; yet Gore more than once on Cook's first voyage had his commander altering course and wasting time looking for land that only one man had sighted, and that man Gore; for Cook was not going to have it said that he had let go anything once seen. Gore had leave while the second voyage was in progress. He sailed again on the third, and ultimately brought the expedition home. In the last days of May 1778 the ships were in Cook Inlet, on the northwest coast of America, and Cook was angry with himself for spending a fortnight there looking for a passage into the Bering Sea, 'very much against my own opinion and judgment', but in deference to the opinion of 'some of the Officers'. One of the officers stood out when everybody else was convinced otherwise, and though Gore is not named, everything points to him; and Cook spent more time to satisfy him. He knew Gore, and it seems to me that he showed patience here, even if the geographical situation
was causing him a good deal of impatience. This is the more remarkable, in that his modesty rarely kept him from arriving at his decisions unaided. When he made dry biscuit the alternative to walrus meat, and cut off the grog in reply to mutiny and turbulence over the sugarcane beer, he may have showed impatience, or he may merely have acted as a disciplinarian in the cause of progress. Of course, in the end the tightly stretched string of patience broke, and Cook died.

These, you may say, are large things, aspects of Cook the great man. But where are more of the small things that make us feel in touch with someone frail and therefore humanly comprehensible like ourselves: where is—really—Cook the man? One has to answer that if Cook had had all the necessary frailties, we should not be putting the query. The aspects of the great man—I say what I have already said—are the things that make the man. I think, on the other hand, we can find that he did not live with utter continuity on the pinnacle of moral and intellectual endeavour. He has his little fragments of self-righteousness. Take his passion for accuracy, in observation and statement. Boswell dined with him once at the house of Sir John Pringle, the President of the Royal Society, and Boswell agreed with Sir John that Cook was a plain, sensible man with an uncommon attention to veracity. Sir John told Boswell how Cook was supposed by one eminent scholar to have seen a nation of men like monkeys, and Cook had corrected the report: 'No', he said, 'I did not say they were like monkeys. I said their faces put me in mind of monkeys.' Boswell comments, 'There was a distinction very fine but sufficiently perceptible'; and 'My metaphor was that he had a balance in his mind for truth as nice as scales for weighing a guinea'.

Now what Cook says in his journal, truly enough, about the people of Malekula in the New Hebrides, is that they 'have Monkey faces'—but he also refers to 'this Apish Nation'; so he may very well have conveyed a clear impression that he had seen a nation of men like monkeys, whatever he thought he had said. Look at another little matter. When he was at Queen Charlotte Sound on his first voyage, he and Green had been careful in settling the situation of the place; when he came back in 1773 and 1774 he was much mortified to find from the observations of the astronomers Bayly and Wales that they had been wrong, and that he had consequently laid down the whole of the South Island of New Zealand on his chart 40' too far east, and the North Island something like 30'. The working of his mind between his log for 7 June 1773, and some date in 1775 or 1776, when he made his final revision of his journal, is interesting and even a bit comic for its struggle with the unpleasant truth—as if he were fascinated by a false accusation, compelled to harp on it all the way across the Pacific, and
finally driven to admit its validity. It was perhaps lucky that he had a very high respect for Wales. He begins,

errors as great as this will frequently be found in such nice observations as these, Errors I call them tho' in reality they may be None but only differences which cannot be avoided.\textsuperscript{27}

It would not much affect either geography or navigation, yet he thought he ought to mention it. He writes in his journal for November 1774, almost with the effect of a generous concession to Wales,

As M\textsuperscript{r} Wales had made so many observations in the Sound for determining the Longitude, I thought it was proper to say what the results were, otherwise I should hardly have mentioned these errors; from a supposition that few will think them of such consequence as either to affect Navigation or Geography.\textsuperscript{28}

Twelve months or so later still and the wound has healed, the scar is gone. True it may be that navigation and geography stand much as they did before; but

I mention these errors \ldots because I have no doubt of their existance, for from the multitude of observations which M\textsuperscript{r} Wales took the situation of few parts of the world are better ascertained than that of Queen Charlottes Sound.\textsuperscript{29}

Note that he had said exactly the same thing of the situation of New Zealand as a whole in 1770; and that he said the same of a number of other places. If Cook had a little pet vanity, it was probably over his fixing of positions. We may like to think he had at least this one, to endear him to us other children of mortality.

How well educated was he?—to ask a question almost at random. His understanding was strong and perspicacious, King tells us, rather superfluously; for anyone can see that. His general knowledge was extensive and various, in that of his own profession he was unequalled, says Samwell. Of course, we answer again, to the second part of this statement, taking his profession as discoverer and maritime surveyor, and not just as a naval person—in which case we might wonder how as a post-captain he would have fought his seventy-four, or as an admiral set about a fleet action. Where, however, did this extensive and various general knowledge take him? He was 'of an agreeable and lively conversation'. What did he talk about, apart from icebergs and men with faces that put him in mind of monkeys? What did he read, apart from text-books in trigonometry and astronomy? We know he read Anson's Voyage and Hakluyt, Maskelyne's \textit{British Mariners' Guide} and Alexander Dalrymple's \textit{Historical Collection of the several Voyages and
Discoveries in the South Pacific Ocean, and Samuel Dunn’s Navigator’s Guide to the Oriental or Indian Seas; but did he ever venture on works of the imagination, apart from Dalrymple’s? Did his friend William Wales spout Thomson’s Seasons to him in Dusky Sound, or offer to lend him that well-thumbed book? We do not know. He wrote a good vigorous common-sense factual prose himself. He was modest about it, and was certainly susceptible to what he considered the superior merits of men like Banks and Wales. He used their journals, as he used Anderson’s, generally to round out his own, not often—though certainly once or twice—for purposes of adornment. We may take it that he had picked up a great deal of various general knowledge simply from the application of his strong and perspicacious mind to his own experience, we may take it that his leanings and education were broadly scientific, were centred on geography and navigation, but that he could cope with most of the common intellectual interests of the day. I have no doubt that a great deal rubbed off on him from men like Banks and Anderson, quite apart from the free use he made of their journals. We may be all the more surprised, then, at the tale the shocked Lieutenant King, who had been appointed to the third voyage, and had called, all excitement and respect, on the great Captain, brought to Forster. King, with some Oxford training, was to share the astronomical work with Cook, and said he was sorry there was to be no scientist on the voyage. I, for my part, am sorry to have to repeat Cook’s reply, for these are no words to utter in an Academy of Science. His words were, ‘Curse the scientists, and all science into the bargain’! At least, so we are told by Forster, and it was on hearing them that he set himself to comfort the young man by explaining Cook’s character. What it does not seem to have struck Forster as necessary to explain was his own effect, as the representative of science, on Cook; what King did not know, in addition, was how much Cook had suffered from Banks, the natural historian with a swollen head, in between voyages. And what King did not yet understand was the composition of the patience and the impatience in his captain’s mind. Literature, learning, science: can we say that he was devoted to any of the arts? We cannot. He was appreciative of the efforts of Hodges, whom he thought an ‘indefaticable gentleman’, on the second voyage:

The Views are all by Mr Hodges and are so judiciously chosen and executed in so Masterly a manner as will not only show the judgment and skill of the artist but will of themselves express their various designs.

In other words, Hodges was a good hand at a landscape, and Cook had no capacity for the higher reaches of criticism. He found the nose-flute in Tonga harmonious, and he loosed the bagpipes on the Pacific;
but that, we may say, was because he had them, not from a connoisseur's devotion.

So far as I can see, he had no religion and no politics. He mentions Providence once or twice. As for the world, the flesh, and the devil, that trio of embarrassments to all good men, he probably had his own definition of them, as no doubt most good men have. The world was something to be explored. With a capital W, it was some sort of embodiment of wrong-headedness that failed to understand the problems of seamen. If it was society, it also had a pleasanter and more reliable side, where a seaman could safely trust himself. It was dinner at Sir John Pringle's, and apparently more regular gatherings of sufficient conviviality. 'Cook is returned, and has resumed his place at the Mitre', writes Daniel Wray, F.R.S., the antiquary, to Lord Hardwicke—the Mitre being the tavern where the Royal Society dined; and the indications are that Cook was a clubbable man. The devil was probably scurvy, and the malign influence that induced sailors not to wash their hands, and sloppiness of mind, and he was opposed to all of them. His ideas about the flesh were orthodox, and his judgments were tolerant. We have seen that he had a proper interest in food. He is said to have regularly proposed, on Saturday nights at sea, the toast of all beautiful women; but no one claims ever to have seen him drunk. He fell in love with Miss Elizabeth Batts soon after he landed in England from the war, and wasted no time in getting married to her; and after that he had no time to waste on Polynesian princesses, or commoners either. They were not neglected: his officers and men were assiduous enough. No doubt there were many young women, and fathers of young women, who would have been glad to have captured such a key fortress, with its store of shirts and spike-nails and axes. Their chances were nil. The reputation Cook earned in this matter in Polynesia may have been one not so much of iron disdain as of physical decrepitude. Truly that would have been so if everybody adopted the attitude of the matron of Nomuka, a small island in Tonga, who, when he declined the offer of a handsome girl, even on credit, berated him soundly as broken-down, old, and good for nothing, and asked him what right he had to insult a young person like that? Or, at least, that was what he made of the tirade. Cook, we have to persuade the movie moguls—I re-emphasise the point—was a professional man. His time was fully taken up. His chief care relating to women in the island groups was to hold back from them, and their societies, the venereal curse that was planted in his sailors. Here, too, he was stubborn, valiant, and humane. It was another battle that he lost.

We can say, then, that his affections were domestic ones, little though the time was he had in which to practise them. He had time to father
six children, only one of whom survived into adulthood. At home at Mile End, in the little house with a garden where Boswell visited him, he seems to have preserved a uniform good temper. Mrs Cook protested against one of the portraits that it made him look stern, and Mr Cook never looked stern. He had ambitions for his sons, and the two eldest, James and Nathaniel, were to go into the navy. This led him to a mild dishonesty. We are at first puzzled by the appearance of a James and Nathaniel Cook in the muster books of the *Endeavour* and *Resolution*, joining the crew as able seamen after the ships had left England. They are the two sons, who enter the service, on paper, at the age of six and five years respectively. They could thus ‘earn time’, as it was said—that is, put behind them the number of years a midshipman had to serve before he could take his lieutenant’s examination. In due course the certificate would be forthcoming. It was a conventional piece of chicanery in the navy; some distinguished men owed their early advancement to it. With Cook it is an index, I suppose, to parental love, and it also shows how far he had come since he volunteered into the navy at Wapping, in June 1755, to try what his fortune would bring that way.
It may be unusual to describe Cook as a scientist, but the title is not unjustified. He was elected a Fellow of the Royal Society, and he was awarded its Copley Medal for his work on improving the health of seamen. Above all, however, he was a scrupulously careful observer; he had a passion for observation of all sorts: birds, animals, people, tides, magnetic variation, everything. His attitude to measurement and inquiry would do credit to any conventionally-trained scientist.

One example must suffice. In June 1770 the *Endeavour* was sailing up the east coast of Australia, inside the Great Barrier Reef. Cook was fully conscious of the dangers, and he had had a leadsman continuously on duty for weeks past; but he was anxious to survey the coast. On 10 June, at 11 p.m. and without warning, the ship struck the Reef, and stuck fast. The sails were at once taken in, and the boats hoisted out; and every effort was made to refloat the ship. Unfortunately, the *Endeavour* had struck the Reef at about high water, and as the immediate attempts to free the ship failed, attention was directed to the next high tide, twelve hours later. The ship was lightened by about forty or fifty tons by jettisoning six of the guns, ballast, and decayed stores; and the men were kept continuously at the pumps. To everyone’s surprise and disappointment, however, the high tide at 11 a.m. was nearly two feet lower than that of the evening before, and the ship could not be dislodged. There was nothing for it but to continue to lighten the ship and to wait for the next high tide.

Joseph Banks recorded in his Journal: ‘Now in my own opinion I entirely gave up the ship and packing what I thought I might save prepar’d myself for the worst’. The officers, he wrote, ‘beav’d with inimitable coolness void of all hurry and confusion’, and indeed, opportunity was taken at noon to determine the latitude of the ship. Banks added:

> All this time the Seamen work’d with surprizing cheerfulness and alacrity; no grumbling or growling was to be heard throughout the ship, no not even an oath (tho the ship in general was well furnish’d with them as most in his majesty’s service).
As the evening tide approached the water rose higher and higher until the coral, which at the morning tide had been at least a foot above water, was wholly covered; and at 10.20 p.m. the ship was hove off into deep water. A few days later, on 17 June, the ship was successfully beached for repairs at the mouth of the Endeavour River.

Most ships' captains would have been satisfied to leave it at that, despite the Admiralty instructions to observe the 'course of the tides and currents'; but Cook was different. He had the curiosity which is the mark of the scientist. In the weeks which followed, he took every opportunity to measure the rise and fall of the tides; and he later published a summary of his observations in the Philosophical Transactions.² He was able to establish the fact that, with the spring tides, the evening tide rose 9 ft while the morning tide rose only 7 ft; and that the low water preceding the evening tide fell considerably lower than the one preceding the morning tide.

This difference in the rise and fall of the tide [he wrote] was uniformly the same on each of the three springs which happened while we lay in the place, and was apparent for about six or seven days; that is, for about three days before and after the full or change of the Moon.

Fig. 5. Part of Cook's chart of the east coast of Australia, showing where the ship struck the Reef, and Endeavour River, 'where we repair'd the Ship' (reproduced from Hawkesworth 1773)
On the other hand, with the neap tides, there was no difference between the morning and evening tides.

No explanation for the diurnal inequality of heights at spring tides could be given at that time, but it was to Cook's credit that he accurately observed and measured the phenomenon.

The known facts of Cook's early life and education are all too few, and there is little to explain his subsequent ability in astronomy and navigation and his scrupulous care as an observer. He went to sea as a young man of seventeen and he learned his seamanship in colliers: three years as an apprentice, about three as a seaman, and about three as mate. He volunteered for the Royal Navy as an able seaman on 17 June 1755, when he was twenty-six, and was posted to the 60-gun ship, *Eagle*. Within a few weeks he was appointed a master's mate.

A master in the Royal Navy was a warrant officer; and the *Eagle*'s master was Thomas Bisset. He was responsible for the navigation and much of the general running of the ship. The master's mates were petty officers who assisted the master in these tasks.

Cook's first tour of sea-duty in the *Eagle* was under the command of Captain Joseph Hamar; but Hamar was soon succeeded by Captain Hugh Palliser, an able officer who was later to become Commander-in-Chief at Newfoundland, and then Comptroller of the Navy. Later still he was to become a Baronet, an Admiral of the White, and one of the Lords of the Admiralty. He developed a high regard for Cook, and did much to advance his interests.

With the help of Palliser and Bisset, Cook progressed rapidly, and on 29 June 1757 he obtained his Trinity House certificate in pilotage and practical seamanship. This qualified him 'to take charge as Master of any of His Majesty's Ships from the Downs thro' the Channel to the Westward and to Lisbon'. The next day he was discharged from the *Eagle*, and appointed Master of the *Solebay*. A few months later, on 27 October 1757, his twenty-ninth birthday, he joined the new 64-gun ship, *Pembroke*.

The *Pembroke* had a short period as part of the fleet cruising off Cape Finisterre and in the Bay of Biscay, and was then assigned to the fleet, under Admiral Boscowen, which was to attack the strongly-defended French fortress of Louisberg. However, owing to an outbreak of scurvy, the *Pembroke* spent some time at Halifax to allow the crew to recuperate, and arrived at Louisberg four days after the attack had begun.

Cook's service in the *Pembroke*, and his arrival in North American waters, may be regarded as the turning point in his educational development. For one thing, the *Pembroke* was under the command of an intellectual, Captain John Simcoe; and secondly, he was to meet and collaborate with the military surveyor, Major Samuel Holland.
It was Simcoe who persuaded Cook to study mathematics and astronomy, and helped him with difficult passages in the text-books. In particular he lent Cook books by Charles Leadbetter, whose works included *A Compleat System of Astronomy* (1728), and *The Young Mathematician's Companion* (1739). The *Pembroke* spent the severe winter of 1758-9 at Halifax, and Cook used the available time for detailed study.

Cook had met Holland immediately following the fall of Louisberg (27 July 1758). The details of the meeting, and the subsequent collaboration between the two, are provided in a letter written by Holland some years later, in 1792, to Simcoe's son, John Graves Simcoe, who had just been appointed Lieutenant-Governor of Upper Canada. The letter ran as follows:

Quebec, 11th January, 1792.

Lt.-Governor Simcoe, York:

Sir,—It is with the most sincere pleasure that I recall to memory the many happy and instructive hours I have had the honor of enjoying in your late most excellent father's company, and with more than ordinary satisfaction do I recollect the following circumstance which gave birth to our acquaintance. The day after the surrender of Louisbourg, being at Kensington Cove surveying and making a plan of the place, with its attack and encampments, I observed Capt. Cook (then master of Capt. Simcoe's ship, the *Pembroke* man-of-war) particularly attentive to my operations; and as he expressed an ardent desire to be instructed in the use of the Plane Table (the instrument I was then using) I appointed the next day in order to make him acquainted with the whole process; he accordingly attended, with a particular message from Capt. Simcoe expressive of a wish to have been present at our proceedings; and his inability, owing to indisposition, of leaving his ship; at the same time requesting me to dine with him on board; and begging me to bring the Plane Table pieces along. I, with much pleasure, accepted that invitation, which gave rise to my acquaintance with a truly scientific gentleman, for which I ever hold myself much indebted to Capt. Cook. I remained that night on board, in the morning landed to continue my survey at White Point, attended by Capt. Cook and two young gentlemen whom your father, ever attentive to the service, wished should be instructed in the business. From that period, I had the honor of a most intimate and friendly acquaintance with your worthy father; and during our stay at Halifax, whenever I could get a moment of time from my duty, I was on board the *Pembroke* where the great cabin, dedicated to scientific purposes and mostly taken up with a drawing table, furnished no room for idlers. Under Capt. Simcoe's eye, Mr. Cook and myself compiled materials for a chart of the Gulf and River St. Lawrence, which plan at his decease was dedicated to Sir Charles Saunders; with no other alterations than what Mr. Cook and I made coming up the River. Another chart of the River, including
Chaleur and Gaspe Bays, mostly taken from plans in Admiral Durell’s possession, was compiled and drawn under your father's inspection, and sent by him for immediate publication to Mr. Thos. Jeffreý, predecessor to Mr. Faden. These charts were of much use, as some copies came out prior to our sailing from Halifax for Quebec in 1759. By the drawing of these plans under so able an instructor, Mr. Cook could not fail to improve and thoroughly brought in his hand as well in drawing as protracting, etc., and by your father’s finding the latitudes and longitudes along the Coast of America, principally Newfoundland and Gulf of St. Lawrence, so erroneously heretofore laid down, he was convinced of the propriety of making accurate surveys of those parts. In consequence, he told Capt. Cook that as he had mentioned to several of his friends in power, the necessity of having surveys of these parts and astronomical observations made as soon as peace was restored, he would recommend him to make himself competent to the business by learning Spherical Trigonometry, with the practical part of Astronomy, at the same time giving him Leadbitter’s works, a great authority on astronomy, etc., at the period, of which Mr. Cook assisted by his explanations of difficult passages, made infinite use, and fulfilled the expectations entertained of him by your father, in his survey of Newfoundland: Mr. Cook frequently expressed to me the obligations he was under to Captain Simcoe and on my meeting him in London in the year 1776, after his several discoveries, he confessed most candidly that the several improvements and instructions he had received on board the Pembroke had been the sole foundation of the services he had been enabled to perform.

With this additional help the 1758-9 winter was a productive study period. Captain King, writing after Cook’s death, confirmed that it was ‘during a hard winter he first read Euclid, and applied himself to the study of mathematics and astronomy’.

When the ice began to melt, the preparations for the attack on Quebec were intensified. Cook and the other masters had the task of charting and surveying the St Lawrance River, and buoynig the dangerous channels. Cook’s charting of the notorious Traverse, near Quebec, was particularly important, facilitating the advance of the fleet; and in due course the Battle of the Plains of Abraham was won.

Cook was then transferred to the Northumberland, under the command of Lord Colville, the new Commodore of the North American station; and for the next few years he continued his surveying work. At one time he worked with J. F. W. Des Barres, a surveyor who had been an aide-de-camp to General Wolfe. The war ended in 1762, and in 1763 he was given the command of the schooner, Grenville, to continue his surveys. It is also of interest to note that Palliser became the Governor
and Commander-in-Chief of Newfoundland in 1764, and it was his influence which led the Admiralty to give Cook permission to publish the charts which he had prepared.

Then, on 5 August 1766, Cook observed a solar eclipse at one of the Burgeo Islands, off the coast of Newfoundland. Using a brass telescopic quadrant made by John Bird, and with the assistance of two others, each with a good telescope, he determined the apparent times of the beginning and end of the eclipse. His results were sent to Dr J. Bevis, who was able to compare the times with those observed for the same eclipse at Oxford, and by this means obtained the longitude of Cook’s place of observation. Dr Bevis wrote a short paper on the observations, which was published in the *Philosophical Transactions*, and he described Cook as ‘a good mathematician, and very expert in his business’. Cook’s results gave the longitude of the island as 57° 31’ W of London (i.e. of St Paul’s); it is now known to be 57° 40’ W of Greenwich, or 57° 34’ W of London. Incidentally, the *Endeavour* was the first ship employed on discovery to use longitudes based on Greenwich, although charts with the prime meridian through Greenwich had been published as early as 1738.

It was with this background of experience that Cook was chosen to command the *Endeavour* and to be one of the Royal Society’s observers of the transit of Venus.

The orbit of Venus around the Sun lies inside that of the Earth, and her path lies directly in front of the Sun only rarely. Four transits of Venus occur every 243 years, at successive intervals of 8, 105½, 8 and 121½ years; and only five transits have ever been observed. The first was on 4 December 1639, when a young English curate, Jeremiah Horrox (or Horrocks), and his friend, William Crabtree, succeeded in observing part of the transit.

At that time the usefulness of the transit as a means of determining the distance of the Sun from the Earth was not known; but in 1716 Edmond Halley published an important paper in the *Philosophical Transactions*, explaining how this could be done. Halley’s plan was that the beginning and end of a transit be exactly determined at two stations remote from one another on the Earth. From the duration of the transit at the two stations, and their known distance apart, the distance of the Sun from the Earth could be calculated.

Halley’s paper provided the stimulus for subsequent observations of the transit. The next transit occurred on 6 June 1761, and there were 120 observers; and the next transit after that occurred on 3 June 1769, when there were 151 observers. Subsequent transits occurred on 9 December 1874 and on 6 December 1882; and the next two will occur on 8 June 2004, and on 6 June 2012.
The results of the 1761 observations were disappointing. Unexpected difficulties were encountered in determining the exact instant of contact between the dark Venus and the bright disc of the Sun. The two appeared to cling together, or to give a 'black drop' or ligament. The results of the various observers differed considerably, and the uncertainty of the distance of the Sun from the Earth remained. It was concluded that further expeditions should be sent to different parts of the world to observe the 1769 transit.

The Council of the Royal Society petitioned King George III to provide a ship to convey observers to a suitable place in the Southern Hemisphere. The petition was approved by the Council on 15 February 1768; and on 5 March the Admiralty wrote to the Navy Board to say that the King had approved the project and to ask the Navy Board to provide a suitable ship. There was some correspondence between the Navy Board and the Admiralty about possible ships; eventually, on 29 March, the Navy Board reported that 'A Cat-built Bark in Burthen 368 Tons, 3 years 9 months old, has been purchased'. This was the Earl of Pembroke; and on 5 April the Admiralty resolved that she 'be registered on the List of the Navy as a Bark by the name of the Endeavour'.

In the meantime, thought was being given to the command of the ship. The Royal Society believed that Alexander Dalrymple (one of the chief exponents of Terra Australis) would be suitable; but the Admiralty refused to give the command of a Royal Navy ship to a civilian, and Dalrymple refused to take part in the expedition unless he was given 'the total management of the Ship intended to be Sent'.

Cook, then Master of the Grenville, had returned to England on 15 November 1767; and it will be recalled that the Admiralty asked the Navy Board to find a suitable ship on 5 March 1768. There is some evidence that Cook had been chosen to command the Endeavour by 12 April, that is within two weeks of the purchase of the vessel.

Cook was introduced to the Council of the Royal Society on 5 May 1768 by Captain John Campbell, a member of the Council who had been round the world with Anson, and who was a friend of Sir Edward Hawke, the First Lord. Campbell told the Council that Cook 'will be appointed by the Admiralty to the Command of the vessel destined for the Observations in the Southern Latitude', and suggested that he 'was a proper person to be one of the Observers'. Cook was then called in and accepted 'the office in Consideration of Such a gratuity as the Society should think fit & £120 a year for victualing himself'. Charles Green, an astronomer who had been an assistant to Nevil Maskelyne, the Astronomer Royal, was also called in and 'accepted the employment of Observer & agreed to the allowance aforesaid with
the farther gratuity to himself of 200 Guineas for the Voyage or if it should exceed two years then 100 Guineas a year'.

The next Council meeting was on 19 May, and Cook's gratuity was fixed at 100 guineas. The following day Captain Samuel Wallis returned to England in the *Dolphin* after his voyage round the world, and announced the discovery of an island (Tahiti) which he had named King George III Island. The position of the island had been fixed with accuracy; the climate was good, and the natives friendly. The island also had a suitable harbour in Port Royal (Matavai Bay). Two weeks later, when the Council of the Royal Society met again, it was decided to ask the Admiralty to send the ship to Tahiti to observe the transit; and at the same meeting it was also decided to request passage for Joseph Banks and for seven members of his staff.

Cook was commissioned as Lieutenant in command of the *Endeavour*, and he went aboard on 27 May 1768. He spent some time provisioning the ship and then sailed for Tahiti, via Cape Horn, arriving in Matavai Bay on 13 April 1769. He chose the north point of the Bay as the place

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**Fig. 6. Plan of Fort Venus at Tahiti, redrawn from a plan prepared by Cook, showing:**

a. Mr Banks's tents; b. the observatory; c. the clock; d. the officers' tent; e. the men's tent and guard room; f. the magazine; g. oven and cook room; h. smith's forge; i. 'necessary house'; k. carriage guns; l. swivels; m. cooper's and sailmaker's tent
for the observations, and built a small fort, between the shore and a small river, to protect the observers and the instruments from the natives. This he called Fort Venus. By 10 May it was finished; all the instruments (many of which had been provided by the Royal Society, and which had been signed for by Cook and Green) had then been set up in proper order:

The astronomical clock, made by Shelton and furnished with a gridiron pendulum, was set up in the middle of one end of a large tent, in a frame of wood made for the purpose at Greenwich, fixed firm and as low in the ground as the door of the clock-case would admit, and to prevent its being disturbed by any accident, another framing of wood was made round this, at the distance of one foot from it. The pendulum was adjusted exactly to the same length as it had been at Greenwich. Without the end of the tent facing the clock, and 12 feet from it, stood the observatory, in which were set up the journeyman clock and astronomical quadrant: this last, made by Mr. Bird, of one foot radius, stood upon the head of a large cask fixed firm in the ground, and well filled with wet heavy sand. A sentinel was placed continually over the tent and observatory, with orders to suffer no one to enter either the one or the other, but those whose business it was. The telescopes made use of in the observations were—Two reflecting ones of two feet focus each, made by the late Mr. James Short, one of which was furnished with an object glass micrometer.15

There was good reason for Cook's caution. The astronomical quadrant had been taken on shore on 1 May, and by the next morning it had been stolen. As Cook wrote in his Journal, 'it was a matter of astonishment to us all how it could be taken away, as a Centinal stood the whole night within 5 yards of the door of the Tent where it was put together with several other Instruments. . . . 516 With some difficulty the quadrant was recovered; some damage had been done, but it was repaired by Herman Spöring, one of Banks's assistants who had worked as a watchmaker in London.

The quadrant was used to check the rate of the astronomical clock, and the rate compared with that at Greenwich was used to give the relative force of gravity at the two places. The latitude was determined by no less than 21 meridian altitudes of the sun, and 27 observations of stars. These gave 17° 29' 15" S as the latitude of the observatory; but Nevil Maskelyne in a footnote in the paper published by the Royal Society (1771) commented:

> It must be confessed, that the results of these observations (most of which were made by Mr. Green) differ more from one another than they ought to do, or than those made by other observers, with quadrants of the same size, and made by the same artist, the cause of which, if not owing to want of care and address in the observer, I don't know how to assign.
Cook did not take kindly to this criticism. In the Journal of his second voyage he commented as follows (but his comments were deleted by the editor, Canon Douglas, when the Journal came to be published):

Mr M. might have assigned another reason, he was not unacquainted with the quadrant having been in the Hands of the Natives, pulled to pieces and many of the parts broke, which we had to mend in the best possible manner we could before it could be made use of.

Mr M. should have considered, before he took upon himself to censure these observations, that he had put into his hands the very original book in which they were written in pencil only, the very moment they were taken and I appeal to Mr M. himself, if it is not highly probable that some of them, might from various causes, be so doubtful to the observer, as either to be wholly rejected or to be marked as dubious and which might have been done had Mr Green taken the trouble to enter them in the proper book. Mr M. should also have considered that this was, perhaps, the only original papers of the kind ever put into his hands; does Mr M. publish to the world all the observations he makes good or bad or did he never make a bad observation in his life.17

Nevertheless, Cook himself criticised the way in which Green had recorded the results of his observations. In a letter written to the Royal Society immediately after his return to England he commented:

We left Batavia on the 26th December, and on the 29th of January Mr Green died; on this unfortunate event, my first care was, to preserve, for the perusal of the Royal Society, all his papers that contained any Astronomical observations of what nature soever; many of which I had never seen before; and which I found far from having been kept in that clear order their importance seemed to require, and the time there was to do it in.18

Green and Cook also determined the longitude of Fort Venus using the method of lunar distances. The altitude, or zenith distances, were all taken with the astronomical quadrant; the distances of the moon from the sun or from the fixed stars were observed with a brass Hadley’s sextant, made by Jesse Ramsden, a particularly skilled instrument-maker who (some years later) was elected a Fellow of the Royal Society. The paper published by Green and Cook in the Philosophical Transactions gives the details of fifty-seven such observations. The mean of all these gave the longitude as 149° 36' 38" W of Greenwich. Opportunity was also taken to observe the eclipses of Jupiter’s satellites; the times, compared with those in the Nautical Almanac (and subsequently corrected by actual observation) for Greenwich, also gave the longitude of Fort Venus; this method gave 149° 32’ 30” W. The Journal
itself, however, records the position of Fort Venus as 17° 29′ S and 149° 30′ W; the latitude is correct, the longitude is now known to be 149° 29′ W.

These observations were subsidiary to those of the transit itself. The day of the transit (3 June 1769) was as favourable as they could wish: not a cloud was to be seen the whole day, and the air was perfectly clear. Green and Cook each observed the transit with a reflecting telescope of 2 ft focus and with a magnifying power of 140; Solander observed the transit with a 3 ft reflecting telescope. Each recorded the times of contact and egress, but

we very distinctly saw an Atmosphere or dusky shade round the body of the Planet which very much disturbed the times of the Contacts particularly the two internal ones . . . we differ'd from one another in observing the times of the Contacts much more than could be expected.20

Further details were given in the paper published by the Royal Society:

At the total egress I found it difficult to distinguish Venus's limb from the penumbra; which of course made the second external contact a little doubtful, and the precise time that the penumbra left the Sun could not be observed to any great degree of certainty, at least by me. Some of the other gentlemen, who were sent to observe at different places, saw at the ingress and egress the same phenomenon as we did; though much less distinct, which no doubt was owing to their telescopes being of a less magnifying power; for the penumbra was visible through my telescope during the whole Transit; and Dr. Solander, whose telescope magnified more than ours, saw it, I have reason to think, distincter
than either Mr. Green or myself; though we both of us saw enough to convince our senses, that such a phenomenon did indisputably exist, and we had a good opportunity to observe it, for every wished-for favourable circumstance attended the whole of that day, without one single impediment, excepting the heat, which was intolerable: the thermometer which hung by the clock and was exposed to the sun as we were, was one time as high as 119°. The breadth of the penumbra appeared to me, to be nearly equal to $\frac{1}{4}$th of Venus's semidiameter.

The success of the method in deriving the distance of the Sun from the Earth was therefore limited (but see Chapter 7); and observations in 1874 and 1882 were to fare little better. Nevertheless it was an experiment well worth doing.

Fig. 8. External and internal contacts of Venus with the Sun, as seen from different places on the Earth

The report to the Royal Society included the determination of the diameters of Venus and of the Sun; and observations were included on the use of the Dip Needle (it recorded 30° 43' at Fort Venus), and on the tides. Cook found, for example, that the difference between low water and high water was only ten inches at Matavai Bay.

In mid-July, all the observations having been made, and many notes on the customs of the natives recorded, the Endeavour began to explore the South Pacific. The success of this part of the voyage was remarkable, leading to a chart of New Zealand and the discovery of the whole of the east coast of Australia. It was a long journey, which was remarkable in this sense also: scurvy for the first time in such a long voyage was relatively unimportant. In this respect Cook's second and third voyages were equally remarkable, and for this Cook deserves considerable credit.
In the eighteenth century the diet of the man at sea left a good deal to be desired. According to *Admiralty Regulations* of the time each man was allowed 1 lb biscuits and a gallon of beer every day, 2 lb beef on Tuesdays and Saturdays, 1 lb pork on Sundays and Thursdays, half a pint of pease on Sundays, Wednesdays, Thursdays, and Fridays, one pint of oatmeal, 2 oz butter, and 4 oz cheese on Mondays, Wednesdays, and Fridays. In addition, fresh fish was distributed as caught. When beer could not be obtained the men were given half a pint of rum, brandy, or arrack; but in the ordinary way 'they drank beer for breakfast, beer for dinner, beer for supper, and beer at all other times when they could get it'.

Some of these provisions were far from appetising. With reference to 'biscuit', Banks was to write in his Journal:

> Our bread indeed is but indifferent, occasioned by the quantity of Vermin that are in it, I have often seen hundreds nay thousands shaken out of a single bisket. We in the Cabbin have however an easy remedy for this by baking it in an oven, not too hot, which makes them all walk off, but this cannot be allowd to the private people who must find the taste of these animals very disagreeable, as they every one taste as strong as mustard or rather spirits of hartshorn.

Taste apart, the food on board was unsatisfactory because of the absence of fresh fruit and vegetables. The diet was deficient in vitamins, especially in vitamin C, and scurvy was therefore a common complaint in seamen whenever the voyage was of more than a few weeks' duration.

Anson's voyage around the world in the years 1740-4 was perhaps worse than most in that, within a year, the crews of his ships had been seriously reduced. Anson's own ship, the *Centurion*, left England with about 500 men; when she arrived at the island of Juan Fernandez, off the West Coast of Chile, only 200 remained. The crew of the *Gloucester*, numbering about 400, had been reduced to fewer than 100; and when Captain Saunders in the *Tryal* arrived at the island he informed Anson that of his 100 men he had buried 34, and that of those who lived only he and the Lieutenant and 3 men were fit enough to work the ship. It is true that many of Anson's men were pensioners, but even so the effect of scurvy was terribly destructive. Walter described the disease in these words:

> This disease, so frequently attending long voyages, and so particularly destructive to us, is surely the most singular and unaccountable of any that affects the human body. Its symptoms are inconstant and innumerable, and its progress and effects extremely irregular; for scarcely any two persons have complaints exactly resembling each other, and where there hath been found some conformity in the symptoms, the order of
their appearance has been totally different. However, though it frequently puts on the form of many other diseases, and is therefore not to be described by any exclusive and infallible criterions, yet there are some symptoms which are more general than the rest, and, occurring the oftener, deserve a more particular enumeration. These common appearances are large discoloured spots dispersed over the whole surface of the body, swelled legs, putrid gums, and, above all, an extraordinary lassitude of the whole body, especially after any exercise, however inconsiderable; and this lassitude at last degenerates into a proneness to swoon, and even die, on the least exertion of strength, or even on the least motion.

This disease is likewise usually attended with a strange dejection of the spirits, and with shiverings, tremblings, and a disposition to be seized with the most dreadful terrors on the slightest accident. Indeed it was most remarkable, in all our reiterated experience of this malady, that whatever discouraged our people, or at any time damped their hopes, never failed to add new vigour to the distemper; for it usually killed those who were in the last stages of it, and confined those to their hammocks who were before capable of some kind of duty; so that it seemed as if alacrity of mind, and sanguine thoughts, were no contemptible preservatives from its fatal malignity.25

Anson's purpose was to assail Spanish power in the Pacific and to capture the treasure being transported in Spanish ships. In this he was particularly successful. The Centurion returned 'carrying the biggest booty that ever returned to England in a single vessel'.26 Anson himself lived to become First Lord of the Admiralty; many of his officers, Piercy Brett, Augustus Keppel, Hyde Parker, Charles Saunders, and Richard Howe, became admirals; and Keppel, Saunders, and Howe became First Lords.

It was the account of the terrible effects of scurvy on Anson's voyage which persuaded the physician, James Lind, to continue his studies of the disease and to publish his conclusions. Lind was born in Scotland in 1716. He began his medical career in 1731 and from 1739 to 1748 he was surgeon in the Royal Navy. On one occasion during this period his ship, the Salisbury, undertook a cruise of ten weeks, during which time some 80 members of the crew of 350 contracted scurvy. Lind carried out the first controlled dietetic experiment on record to test the efficacy of various antiscorbutic remedies. Using twelve men with scurvy he was able to prove that all the so-called remedies were ineffective except citrus fruit. Two men who were given two oranges and one lemon a day rapidly recovered: one was fit for duty after six days and the other was appointed nurse to the remaining ten patients who received the other supposed remedies.

In 1753 Lind published A Treatise of the Scurvy, a critical review of the
CAPTAIN COOK

disease and its remedies and of his own observations, which he dedi­
cated to Lord Anson. He concluded that it is a deficiency disease, 
caused by the absence of fresh fruit and vegetables in the diet; and 
he recognised the great value of oranges and lemons in preventing or 
curing the disease. He freely admitted that he was not the first to 
recognise the value of such fruit, but he provided far more convincing 
evidence than any previous commentator on the disease.27 Lind 
advocated the regular issue of fresh fruit and vegetables; and he also 
described a method of preserving orange and lemon juice so that it 
could be used on long voyages. This was the ‘rob of oranges and 
lemons’, obtained by boiling down the juice to the consistency of syrup.

Lind’s recommendations were not put into universal practice in the 
Royal Navy until after his death, and even then the results were some­
times uncertain. At times there was some confusion between lime juice 
and lemon juice,28 and the rob of oranges and lemons was of uncertain 
potency. It is now known that aqueous solutions of vitamin C are 
relatively unstable and undergo ready oxidation, so that some of the 
rob produced must have been almost devoid of activity. It is not sur­
prising that Cook found little use for rob.

For these and other reasons the results of the use of the dehydrated 
orange and lemon juice were uncertain. It was criticised and distrusted; 
and new causes and new remedies were suggested from time to time. 
Scott’s antarctic expedition, a century and a half after Cook, was 
provisioned on the basis of the tainted meat theory. The rations of the 
polar party consisted of biscuits, pemmican, butter, cocoa, sugar, and 
tea, with no fruit or vegetables whatsoever.29 As Chick has remarked: 
‘there can be little doubt that Captain Scott and his comrades were 
suffering from scurvy when overtaken by exhaustion and death’.

When Cook provisioned the Endeavour in 1768 there was little under­
standing of scurvy or indeed of nutrition in general. The provisions 
put on board were recorded in the Minutes of the Victualling Board 
as follows:

Bread in Bags 21,226 pounds. Ditto in Butts, 13,440 pounds. Flour for 
Bread in Barrels 9,000 pounds. Beer, in Puncheons 1,200 gallons. 
Spirits 1,600 gallons. Beef 4,000 pieces. Flour in lieu of ditto in half 
barrels, 1,400 pounds. Suet—800 pounds. Raisons—2,500 pounds. 
Pease in Butts 187 Bushells. Oatmeal 10 ditto. Wheat 120 Bushells. 
Oil 120 gallons. Sugar 1,500 pounds.—Vinegar 500 gallons. Sour Krout 
7860 pounds. Malt in Hogsheads 40 Bushells. Salt 20 ditto. Pork 6,000 
pieces. Mustard seed 160 pounds.30

Some of these stores were later increased. For example; 100 gallons 
of arrack was added, and 20 bushels of salt; and 40 lb saloup was also
provided. Additional provisions were obtained during the voyage. At Madeira, Cook obtained 1,228\frac{1}{2} lb fresh beef and 3,845 lb onions; and he also obtained wines and 'other necessaries' for the use of Mr Green and himself. Considerable quantities of food and other supplies were also purchased on the return journey at Batavia, and at the Cape of Good Hope.

It was widely believed that certain foodstuffs would prevent the onset of the disease or even cure it in its early stages. The *Endeavour* was therefore provided with several items which were believed to be effective in this way: rob of oranges and lemons, sauerkraut, portable soup, and malt.

The large quantity of sauerkraut put on board is of interest, and Cook did find it of value once he had persuaded the men to eat it. On his return to England he reported to the Victualling Board:

> You were pleased in the Year 1768 to put on board the Endeavour Bark a quantity of Sour Kroutt in order that a proper Tryal might be made of its Efficacy against the Scurvey and directed me upon my return to report to you how the same shall be found to Answer. I am to acquaint you that Sour Kroutt together with the many other Antiscorbutics my Lords Commrs of the Admiralty were pleased to order to be put on board did so effectually preserve the People from a Scorbutive Taint that not one dangerous case hapned in that disorder during the whole Voyage, and it is the Surgeons, Officers, and my Opinion that Sour Kroutt had a great Share in it and that it will always be found extremely beneficial to Seamen when they are obliged to live long upon a Salt diet; it has the good quality not to loose any part of its Efficacy by keeping we used the last of it in September last after having been above two years on board & it was then as good as at the first.\(^{31}\)

Banks commented in his Journal that he ate the sauerkraut 'till our salted Cabbage was opend which I preferd as a pleasant substitute'. The salted cabbage was prepared with care:

> Take a strong Iron bound cask for no weak or wooden bound one should ever be trusted in a long voyage, take out the head and when the whole is well cleand cover the bottom with salt. Then take the Cabbage and stripping off the outside leaves take the rest leaf by leaf till you come to the heart which cut into four; these leaves and heart lay upon the Salt about 2 or 3 inches thick and sprinkle Salt pretty thick over them and lay cabbage upon the salt stratum super thick till the cask is full. Then lay on the head of the cask with a weight which in 5 or 6 days will have pressd the cabbage into a much smaller compass. After this fill up the cask with more cabbage as directed and Head it up. N.B. the Cabbage should be gatherd in dry weather some time after sun rise that the dew may not be upon it. Halves of cabbages are better for keeping than single leaves.\(^{32}\)
Portable soup was a dehydrated product, and 1,000 lb of this was placed on board for use on Banyan days (i.e. on meatless days). It was prepared for use by boiling 4 oz pease in a quart of water and adding 10 oz portable soup, and was doubtless reasonably nourishing, but was clearly ineffective as an antiscorbutic.

In 1938 Drummond and Macara examined a cake of portable soup which had been manufactured in 1771 and which was believed to have been one of those carried by Cook on his second voyage. The report read:

The cake was a flat, rectangular slab, 110 by 96 by 7 mm. and greyish white in colour. It was stamped with a 'broad-arrow.' At some time it had been broken into two portions by a diagonal crack . . . the edges of the crack had an appearance similar to those of a slab of glue; indeed, the general resemblance to glue was very striking, which is of interest in view of the contemporary description of its preparation. . . The material was very hard and tended to fracture conchoidally just as a slab of hard glue would do. The flakes were quite transparent and a golden brown in colour. The material was readily soluble in cold water, forming a clear, pale yellow solution with only a very slight trace of flocculent insoluble matter. The reaction was acid. . . It was almost devoid of smell and taste. The material seems to be a desiccated clear broth, which in all probability was prepared from meat and bones. It does not seem to have undergone any marked change during 160 odd years.33

Malt was also included among the provisions put on board the Endeavour. Its use for the treatment of scurvy had been recommended by Dr David MacBride in his book, Experimental Essays on the Scurvy and other objects, which was first published in London in 1764. Cook was provided with copies of the book, and given detailed instructions on the use of the malt. One quart of malt was to be ground and added to three quarts of boiling water. The resulting wort was then to be boiled into a panada with sea biscuits or dried fruit, and given to anyone showing symptoms of scurvy.34

Rob of lemons and oranges was provided for the Endeavour and delivered to the surgeon at the office of the Sick and Hurt Board so that instructions for its use could also be given. Cook was not particularly impressed with the virtues of this remedy, however, during either his first or second voyage. His paper to the Royal Society, published in 1776, referred to malt, sauerkraut, and portable soup; and he added: 'Further, we were provided with Rob of lemons and oranges; which the surgeon found useful in several cases'.35 In a later note to the President he commented:

I entirely agree with you, that the dearness of the Rob of lemons and of oranges will hinder them from being furnished in large quantities, but I
do not think this is so necessary; for though they may assist other things, I have no great opinion of them alone.36

Banks had his own supply of rob of oranges and lemons, prepared under the direction of Dr Nathaniel Hulme. One cask contained lemon juice which had been evaporated to one-third of its bulk; a second contained orange juice and brandy; and the third contained lemon juice and brandy. It is not surprising that the latter two were more effective than the material which had been subjected to evaporation; and Banks had this to report in his Journal:

About a fortnight ago my gums swelld and some small pimples rose in the inside of my mouth which threatened to become ulcers, I then flew to the lemon juice which had been put up for me according to Dr Hulmes method describd in his book and in his letter which is inserted here: every kind of liquor which I usd was made sour with the Lemon juice No 3 so that I took near 6 ounces a day of it. The effect of this was surprizing, in less than a week my gums became as firm as ever and at this time I am troubled with nothing but a few pimples on my face which have not deterrd me from leaving off the juice intirely.37

There were certainly some signs of scurvy during the Endeavour voyage; but on arrival at Batavia Cook was able to note that he had not lost a man from this disease. Two had been frozen to death, two had been drowned, one had jumped overboard, one died of alcoholic poisoning, one of epilepsy, and one of tuberculosis. Dysentery and malaria were to claim many men in Batavia and later; but scurvy was never important.

The remedies provided for Cook’s second and third voyages were similar. For the second voyage,38 for example, the Resolution was provided with over 19,000 lb sauerkraut, nearly 5,000 lb salted cabbage, and 3,000 lb portable soup. In addition, she had 30 gallons of a new remedy, known as ‘mermalade of carrots’, recommended by Baron Storsch of Berlin; but, as carrots have very little vitamin C, it is not surprising that it was found to be ineffective.

It is possible that one or two of the remedies had some effect in helping to prevent scurvy; but the reader of the Endeavour Journals of Cook and Banks, and of the various Journals written during the second and third voyages, cannot help being impressed by the stress which Cook laid on fresh vegetables and the need to seek such food whenever the ship touched a suitable shore. There are numerous references in the Journals to the use of wild celery, scurvy grass, cress, wild cabbages, and other materials, ‘all of which we found to eat very well either in Soups or Sallids’;39 and spruce beer was also brewed.
In this connection it is of interest to comment on the Kerguelen cabbage. Cook visited Kerguelen Island on his third voyage, and his surgeon, Anderson, described the cabbage:

I have eaten it frequently raw and found it to be almost like the Scurvy grass of New Zealand, only tenderer, but it seem'd to acquire a rank flavour when boil'd which however some of our people could not perceive and esteemed it good.40

Cook named it *Pringlea antiscorbutica*, after the President of the Royal Society, Sir John Pringle. A recent examination of this cabbage has shown that the heart leaves contain 155 mg per gm of vitamin C; that is, a greater amount than is present in most cabbages.41

Cook himself wrote:

We came to few places where either the art of man or nature did not afford some sort of refreshment or other, either of the animal or vegetable kind. It was my first care to procure what could be met with of either by every means in my power, and to oblige our people to make use thereof, both by my example and authority; but the benefits arising from such refreshments soon became so obvious, that I had little occasion to employ either the one or the other.42

The men did need to be persuaded to eat these unknown vegetables, as Cook himself admitted:

Many of my People, officers as well as seamen, at first, disliked Celery, Scurvy grass &c being boiled in the Pease & Wheat and some refused to eat it, but as this had no effect on my conduct, this obstinate kind of prejudice, little by little wore off and they began to like it as well as the others and now, I believe, there was hardly a man in the Ship that did not attribute our being so free of the Scurvy to the Beer and Vegetables we made use of in Newzealand.43

Alexander Home, who sailed in the *Discovery* on Cook's third voyage, recorded that 'it was the Custom of Our Crews to Eat almost Every Herb plant Root and kinds of Fruit they Could Possibly Light [upon] with[out] the Least Inquirey or Hesitation'.44 But he added that

it was No Uncommon thing when Swallowing Over these Mess[es] to Curse him heartily and wish for gods Sake that he Might be Obledged to Eat such Damned Stuff Mixed with his Broth as Long as he Lived.

On the second voyage Cook's ship, the *Resolution*, remained free from scurvy; but his companion ship, *Adventure*, under Captain Furneaux, had many cases of scurvy at Queen Charlotte Sound. Cook commented in a letter to the Admiralty Secretary that 'they were unacquainted with the method of making Spruce Beer & Strangers to
many of y° Vegetables with which that place abounds’; but the real reason for the difference was to be found in the enthusiasm and leadership provided by the two captains.

From the health point of view Cook’s second voyage was even more remarkable than the first; it was not marred by the deaths due to dysentery and malaria contracted at Batavia. He concluded his paper to the Royal Society, which summarised his methods and observations, in the following words:

These, Sir, were the methods, under the care of Providence, by which the Resolution performed a voyage of three years and eighteen days, through all the climates from 52° North to 71° South, with the loss of one man only by disease, and who died of a complicated and lingering illness, without any mixture of scurvy. Two others were unfortunately drowned, and one killed by a fall; so that of the whole number with which I set out from England I lost only four.

Cook was not a theorist; he was an empirical scientist. His approach was experimental. His contribution to the problem of maintaining the health of seamen can hardly be over-emphasised, and he made real and significant contributions to the development of astronomical navigation. He was outstanding among his contemporaries as an accurate observer. His success as an explorer lay in his curiosity and his insistence on the accurate determination of latitude and longitude by all the means available; and he was always ready to make any number of observations to achieve the desired accuracy. He took great care in the preparation of all his reports and charts; so much so that many charts based on his work were in constant use for over a hundred years. He fostered and encouraged the work of the botanists and others who accompanied him on his voyages—voyages which set the pattern for scientific voyages of discovery in future years.
James Cook was the supreme navigator of the eighteenth century. During his three voyages his ships cruised widely in the Antarctic and Pacific Oceans, and charted for the first time the whole coastline of New Zealand and the east coast of Australia. He disproved the ancient theory of a vast southern continent to balance the Earth.

He lived in an age, inspired by Newton, when astronomers were giving navigation new methods of finding the position of a ship at sea. In his time, too, accurate observations at sea became possible, using the refined instruments being designed and made in England.

Cook was fully familiar with these new opportunities; by his personal work and that of the officers and civilians of his expeditions he proved in practice the value of these new methods. When his third expedition ended with his death in Hawaii, he had firmly established the basis of modern navigation at sea.

The rapidity of the change in the ability of mariners to find their position at sea is vividly portrayed by contrasting the experiences of Anson and Cook near Cape Horn when voyaging to the Pacific.

When Cook was twelve years old, George Anson in 1740 left England in H.M.S. Centurion, in command of a squadron of eight ships and a total of 1,955 men. The purpose of this expedition was to harass the Spaniards on the west coast of the Americas and in the Pacific.

On 27 February 1741 Anson’s squadron departed from St Julian on the east coast of Patagonia, and on 7 March passed through the Le Maire Strait between Staten Land and the eastern point of Tierra del Fuego. Setting a southerly course in very stormy weather, the squadron on 28 March reached a south latitude of 59° 30’. The problem facing the Master of the squadron was to estimate whether the ships were sufficiently far west to round the southern tip of South America.

He had no direct method of measuring longitude; from the inaccurately measured daily run of the ships in stormy weather and after making allowance for an easterly current of unknown extent, he had to decide if the ships had reached sufficiently westward for a safe course to be set to the northwest into the Pacific.
His estimate was considerably in error. On 14 April, when the Master expected the squadron to be 10° to the west of the westernmost point of Tierra del Fuego, early in the morning during a sudden clearing of the weather, land was sighted. Since they were now in the latitude of the Straits of Magellan, this land was identified as Cape Noir on the west coast of Tierra del Fuego.

The consternation of the squadron is vividly told by Richard Walter in his account of Anson’s voyage. Referring to their belief that the squadron was far to the west, he states:

But these were delusions which only served to render our disappointment more terrible; for the next morning, between one and two, as we were standing to the northward, and the weather, which had till then been hazy, accidentally cleared up, the Pink made a signal for seeing land right a-head; and it being but two miles distant we were all under the most dreadful apprehensions of running on shore; which, had either the wind blown from its usual quarter with its wonted vigour, or had not the moon suddenly shone out, not a ship amongst us could possibly have avoided: But the wind, which some few hours before blew in squalls from the S.W. having fortunately shifted to W.N.W., we were enabled to stand to the southward and to clear ourselves of this unexpected danger...

Sailing again to the southwest into latitude 60°, the course was altered to northwest towards the island of Juan Fernandez.

But here again inability to measure longitude brought disaster to the squadron. Anson’s only option was to steer to the northwest, hoping to bring himself into the latitude of Juan Fernandez sufficiently west of the island to enable him to steer eastward along the parallel of latitude until he sighted land.

A dispute arose amongst his officers as to whether the ships were in fact east or west of the islands, and their doubt was resolved only when, after two days’ sailing in the westerly winds, the squadron sighted the tops of the Andes near Valparaiso.

Turning again to the west, and having now to sail into the wind, the ships took ten days sailing to westward to regain their previous position. The island was sighted on 9 June 1741. The delay of ten days in arriving at Juan Fernandez cost the lives of 70 to 80 men in a squadron by then suffering severely from scurvy and other health disabilities.

Cook’s experiences were entirely different. In the twenty-eight years that had elapsed since Anson’s voyage, the method of longitude determination by observations of the Moon and the Sun had become available, and was used by Cook. The Endeavour left Rio de Janeiro on 1 December 1768, and on 11 January 1769, in latitude 50° 20' and longitude 64° 35', sighted Tierra del Fuego. On 13 and 14 January
Cook attempted to pass Le Maire Strait, but without success, owing to adverse southerly winds and storms.

On the afternoon of Saturday 14 January at 6 p.m. he states:

At 6 the Weather being clear took Nine or three sets of observations of the Sun and Moon in order to find the Longitude of the place, and as they perhaps are the first observations of this kind that were ever made so near to the extremity of South America I have inserted them below just as they were taken that every body may judge for themselves, Cape S' Diego bore at this time SBE distant about 4 Leagues.

On 16 January he anchored his ship in the Bay of Success on the east coast of Tierra del Fuego inside the Le Maire Strait and stayed there until the 21st. On the 26th, with some difficulty, he identified the southernmost point of Cape Horn and, as a result of several observations of the Sun and Moon made a day after he left land, he determined the latitude to be 55° 59' S and the longitude 68° 13' W. Departing from the Horn on 26 January, he ultimately reached a south latitude of 60° and longitude approximately 74° before turning northwest into the Pacific.

The contrast is vividly apparent. The uncertainty of the past is gone, and Cook proceeds with the confidence of a skilled navigator always aware of his ship's position and taking many observations for the benefit of others.

Isaac Newton died in 1727; James Cook was born in 1728. Newton’s third book of the Principia, *The System of the World*, was published in English in London in 1728, the year of Cook’s birth. Newton gave to astronomers the theory that allowed the observation of heavenly bodies to be mathematically described but, more importantly, their relative positions in the future to be predicted.

A navigator, on shipboard, must every day keep a continuous 'account' of the course and speed of his ship. The ‘dead reckoning’, as it is called, involved in Cook’s day an hourly measurement of the speed of the ship in knots, using the log line, and the course of the ship, measured by magnetic compass.

A description of the procedure is given in Moore’s *New Practical Navigation* (1804):

In heaving the log, one man holds the reel on which the line is wound and another the 1/2 minute glass; an officer of the watch heaves the log over the lee quarters and when he observes that sufficient line is run off (marked by a red rag) he calls ‘Turn’ and the glass holder says ‘Done’ and when the sand has run out cries ‘Stop’ and the knots and fathoms reeled off are noted.

With these data, the daily run of the ship could be computed from a point of known latitude and longitude, and the new position estimated.
In Cook’s day such measurements were relatively crude and there was always considerable doubt as to the ship’s position by dead reckoning. Moreover, the leeway of the ship and the effect of ocean currents could not be easily determined out of sight of land. In long voyages such as Cook’s into the Pacific, the variation of the magnetic compass from true north was usually unknown, and hence throughout Cook’s voyages many measurements of the variation of the compass by astronomical observations were made.

In an age when long voyages of discovery were much in the minds of the governments of England and France, more precise methods of determining the position of a ship were obviously called for.

For many years it had been known that latitude could be measured by taking the altitude of the Sun above the horizon at noon. If the angular distance of the Sun above or below the equator, or its ‘declination’, was also known, latitude could be simply calculated.

The observation of the altitude of a heavenly body or of the angular distance between two heavenly bodies taken at sea, often under adverse conditions, was subject to considerable uncertainty until the sextant was perfected.

Both Isaac Newton and Robert Hooke had the basic idea of a reflecting instrument suitable for measuring the altitude of a heavenly body at sea. Newton devised a reflecting quadrant which was the forerunner of the more practical instrument of Hadley.

John Hadley, F.R.S., demonstrated his instrument to the Royal Society in 1731. By means of a mirror attached to an arm moving over an arc of 45° the image of the heavenly body could be brought to coincidence with the horizon seen directly through a small telescope. Angles of up to 90° could be measured, and, since an arc of 45° was used, the correct name for this instrument is an ‘octant’.

Jesse Ramsden, F.R.S., noted for his engine for dividing mathematical instruments, contributed to the design and making of Hadley’s octant through his instrument-making business. John Dollond and his son Peter were also prominent instrument-makers at the time; they were paid £168 by the Navy Board for instruments supplied for Cook’s second voyage.

It was Vice-Admiral John Campbell, F.R.S., who made the crucial change to Hadley’s octant. Campbell, who sailed with Anson in the Centurion on the famous voyage around the world, was later ordered by the Board of Longitude to test Mayer’s lunar tables and a quadrant designed by Mayer to allow more accurate readings than were possible with Hadley’s instrument. For measuring the distance of the Sun from the Moon Campbell found the Hadley instrument inconvenient and difficult to use, and he therefore devised a ‘sextant’ reading to 120°. This
was made for him by Bird, an instrument-maker in London, and was the predecessor of the modern sextant of today.

On his first voyage, Cook and his companions had four sextants, made by Bird, Nairne, and Ramsden, and of the accuracy of all these Cook was highly complimentary. In his comments on the *Nautical Almanac* in 1773, he stated:

Much Credit is also due to the Mathematical Instrument makers for the improvements and accuracy with which they make their Instruments, for without good Instruments the Tables would loose part of their use: we cannot have a greater proof of the accuracy of different Instruments than the near agreement of the above observations, taken by four different Sextants and which were made by three different persons, viz: Bird, Nairne & Ramsden.¹¹

The numerous disasters and difficulties of mariners in the age of growing British maritime power made the problem of finding longitude at sea one of urgent and vital importance.

In those days British seamen took the meridian between the north and south poles passing through the Royal Observatory at Greenwich as their zero of longitude. It was not until 1884, as a result of the Washington Meridian Conference, that this meridian was agreed to be the zero of longitude for all mariners. The Earth’s circumference at the equator, when divided into 360°, provides the meridians of longitude of other places, the measurement in degrees being either east or west of Greenwich. Owing to the Earth’s rotation, the Sun passes over any meridian once every twenty-four hours, and thus the longitude difference of two places 15° apart is equal to one hour in time. The ship’s longitude is thus obtained by the difference in local time and the time simultaneously at Greenwich. The problem of finding the longitude of a ship at sea is that of knowing the time at Greenwich.

The British government, in 1714, in the reign of Queen Anne, passed an Act of Parliament offering a substantial reward for a practical and useful method for finding longitude at sea.¹² The proposed method was to be tested during a voyage of six weeks between two places of known longitude. The greatest amount, £20,000, was to be awarded for a method which proved accurate within 30° of longitude (i.e. 30 sea miles). Smaller rewards of £15,000 if the accuracy was within 40 sea miles, and £10,000 if within 60 sea miles, were also offered.

The Act also set up a permanent body of Commissioners charged with supervising all competitions for this award; this body became known as the Board of Longitude.

This substantial prize attracted many proposals, of which the majority were either erroneous in concept or too inaccurate or impractical for
use at sea. The only two practical methods worthy of attention and practised by Cook were the measurement of longitude using what is called lunar distances, and the later method using the chronometer. Only the latter survives today.

The position of the Sun in the heavens gives a measure of local time. Another heavenly body must be used if an astronomical observation is to give the time at Greenwich; the most convenient is the Moon. That the position of the Moon relative to the Sun constantly changes is a common observation; from one full Moon to the next approximately one month elapses. If the relative positions of these two bodies can be predicted for any time at Greenwich, and if these prediction tables are in the hands of a navigator, he can, by an observation of the angular distance between them, tell the time at Greenwich.
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the calculated distances from the Moon to the Sun and selected stars
The mathematician, Euler, using Newtonian principles, had found an approximate solution of the problem of the mutual attraction of three gravitating bodies. Tobias Mayer of Göttingen, and a Fellow of the Royal Society, using this theory, computed the motion of the Moon and prepared lunar tables which were published in London in 1767.13

The fifth Astronomer Royal, Nevil Maskelyne, proved the validity of this method of longitude determination by taking observations on the ship in which he went to St Helena, where he had been sent to observe the transit of Venus.14 The method of using lunar distances which he evolved he described to the Royal Society in 1761. Maskelyne, however, went much further than this; if the method of lunar distances was to be used commonly by mariners at sea, the method not only had to be simplified, but tables predicting the position of the Moon relevant to the Sun were essential.

Maskelyne’s great contribution to navigation was to publish in 1766 the first Nautical Almanac15 and to accompany this by the publication of his British Mariner’s Guide, which described the method of using lunar distances as a navigational aid.

When Cook went to sea on his first voyage in 1768, he was not only fully versed in the practice of longitude determination by this method, but was also equipped with the editions of the Almanac necessary to cover the period of his voyages. His journal shows that he constantly used this method of longitude determination to good effect throughout the first voyage.

Maskelyne endeavoured persistently to convince the Board of Longitude that his method of lunar distances provided the answer to longitude determination, but he did not succeed. Although the method is capable of reasonable accuracy, and was a serviceable method prior to any better method being devised, it did suffer from serious deficiencies. Even with the simplifications that Maskelyne introduced, the calculations were still complex, and moreover, it is evident that the Moon is not always visible or in a suitable position, and is frequently obscured in bad weather.

The alternative method, that of carrying in the ship an accurate timepiece, had been widely considered. The question was whether a timepiece could be designed which would keep time with sufficient accuracy over long periods. This was first solved by John Harrison, who, after prolonged argument, was finally awarded the prize by the Board of Longitude.16 John Harrison had already invented the well-known gridiron pendulum; it is believed that it was in 1728 that he first began to consider the design of a marine clock.

He made, altogether, five timepieces; the first three are large machines now kept in the Royal Maritime Museum at Greenwich; the fourth,
Harrison's No. 4, is in the Royal Observatory at Greenwich; and the fifth is in the Clockmakers' Company Museum, London.

The final and conclusive trials of Harrison's No. 4 were made in 1764. Maskelyne and Green (who later went on Cook's first voyage) were entrusted with the determination of the longitude in Jamaica, against which the accuracy of the No. 4 was to be tested. Harrison's son, William, carried this timepiece in H.M.S. Tartar on the voyage to Jamaica. The mean value of the difference in longitude determined by astronomical observations between Portsmouth and Jamaica was measured as 3 hours, 54 minutes, 18.2 seconds. The timekeeper gave 3 hours, 54 minutes,
50.6 seconds, a total error of 32.4 seconds in seven weeks. This corresponded to 9.6 miles of longitude at the equator.

Before being awarded the prize, Harrison was forced by the Board of Longitude to hand over his No. 4 as the property of the nation and to disclose the method of its construction. The Board then commissioned Larcum Kendal to make an exact copy of H4, for which he was paid £450. This chronometer (K1) was completed in 1769 and delivered to the Board in January 1770. Also in 1770 John Arnold, who is credited
with having developed the manufacture of chronometers in England and bringing them into widespread use, completed his first chronometer (A1). He later made A2 and A3.

Thus by the time Cook departed on his second voyage chronometers were available and were carried in his ships, Resolution and Adventure. William Wales, the astronomer who sailed in Resolution, was entrusted with Kendal's K1 and Arnold's A3, while the astronomer, Bayly, carried in Adventure the two Arnold chronometers A1 and A2.

When Cook left England on his first voyage he was thoroughly familiar with the method of measuring longitude by observing the Sun and Moon, and he personally practised this observation throughout the voyage.

Although Green, the astronomer who accompanied him, was sent principally to observe the transit of Venus in Tahiti, he had been the companion of Maskelyne during the latter's work in evolving the method of lunar distances and testing it on ocean voyages. Green therefore took part throughout the first voyage in the determination of longitude by lunar distances.

Cook pays a tribute to Green in the following terms:

In justice to Mr. Green, I must say that he was Indefatigable in making and calculating these observations which otherwise must have taken up a great deal of my time, which I could not at all times very well spare. Not only this, but by his Instructions several of the Petty officers can make and Calculate these observations almost as well as himself: it is only by such means that this method of finding the Longitude at sea can be put into universal practice—a method that we have generally found may be depended upon to within half a degree; which is a degree of accuracy more than Sufficient for all Nautical purposes.17

Although Cook was not specifically required by his instructions from the Admiralty to do so, the frequent navigational observations of the first voyage provided ample demonstration of this most recent advance in position-finding.

After leaving Tahiti, Cook was instructed to sail south to latitude 40º and then, if he had not discovered a southern continent, to turn westward until he encountered the land named 'New Zeland' discovered by Tasman in 1642.18 Cook carried out these instructions with precision. He reached latitude 40º S on 2 September 1769 and then, on a westward course, found Poverty Bay in New Zealand on 6 October 1769. This, followed by a complete circumnavigation and charting of New Zealand, was the first great discovery of this voyage.

He departed from Cape Farewell on 1 April 1770, and, after a fair-weather crossing of the Tasman Sea, sighted Cape Everard on the
southeastern Australian coast on 19 April. After cruising the whole length of the eastern coast of Australia and surviving the wreck of his ship on the Great Barrier Reef, he proved, by the passage through Endeavour Strait on 23 August, that the mainland of Australia was not part of New Guinea.

In reporting to the Admiralty in his letter to the Secretary, Cook said:

The Charts and Plans I have drawn of the places we have been at were made with all the care and accuracy that time and circumstances could admit of. Thus far I am certain that the Latitude and Longitude of few parts of the world are better settled than these. In this I was very much assisted by Mr. Green who let slip no opportunity for making observations for settling the Longitude during the whole course of the Voyage and the many Valuable discoveries made by Mr. Banks and Dr. Solander in Natural History and other things useful to the learn'd World cannot fail of contributing very much to the Success of the Voyage.

The modesty of this statement is set into its true perspective by the statements made by Flinders, when in 1814 he made his great survey of the Australian coastline, traversing the same areas that had previously been charted by Cook.

In the introduction to the account of his work, Flinders states:

This voyage of Captain Cook, whether considered in the extent of his discoveries and the accuracy with which they were traced, or in the labour of his scientific associates, far surpassed all that had gone before.

Flinders also commented on the accuracy of Cook’s observations:

Timekeepers were in their infancy in 1768, when Captain Cook sailed upon his first voyage and he was not then furnished with them; his longitude was therefore regulated only by occasional observations of lunar distances and some few of Jupiter’s satellites, which even in the present improved state of instruments and tables require to be connected by timekeepers before satisfactory conclusions can be drawn. Errors of greater or less magnitude were thence unavoidable; At Cape Gloucester, where I quitted the East Coast, my longitude was 20 1/3' greater than Captain Cook's chart. At Cape York, where the survey was again resumed, it was 58 1/2 ; and to incorporate the intermediate parts it was necessary not only to carry his scale of longitude 20 1/2' more west, but also to reduce the extent of the coast.

On this voyage Cook made the first of his series of great discoveries and contributions to navigation. He already had further ambitions. At the end of his Journal he had stated his view that:

Now I am on the subject of discoveries I hope it will not be taken a Miss if I give it as my opinion that the most feasible method of making
further discoveries in the South Sea is to enter it by the way of New Zealand, first touching and refreshing at the Cape of Good Hope, from thence proceed to the Southward of New Holland for Queen Charlotte's Sound where again refresh Wood and Water, taking care to be ready to leave that place by the latter end of September or beginning of October at farthest...

When the second voyage had been planned and preparations were well under way, the Board of Longitude received a letter from the Astronomer Royal, Nevil Maskelyne, which, as recorded in the records of the Board, stated in its introduction that the intended expedition to the South Seas

... may be rendered more serviceable to the improvement of geography and navigation than it can otherwise be if the ship be furnished with such astronomical instruments as this Board hath the disposal of or can obtain the use of from the Royal Society and also some of the longitude watches and above all if a proper person could be sent out to make use of those instruments and teach the officers on board the ship the method of finding the longitude.23

This was agreed to by the Board, which, on advice, appointed William Wales, a Fellow of the Royal Society and an astronomer, and William Bayly, also an astronomer, to accompany the voyage.

The instructions to Wales are most comprehensive.24 Amongst the many observations he was instructed to make, he was particularly charged with the care of the ‘watches’ in Resolution, and

you are to Note also the times of the Watches when the Sun’s Morning and Afternoon altitudes or the Distances of the Moon from the Sun and fixed Stars are Observed; and to compute the Longitude resulting from the Comparisons with the Watches with the Apparent time of the day inferred from the Morning and Afternoon Altitudes of the Sun.

This second voyage, which left Plymouth in July 1772, therefore became a grand test of the use of the chronometer on a long voyage. Longitude was still to be computed by observing the position of the Moon relative to the Sun or the fixed stars, but these observations were to be compared with the longitude computed by the use of the chronometer.25

William Wales, in Resolution, played a full part in this test by making observations constantly at sea and by arrangement with Cook on shore at widely separated sites in the southern hemisphere. Cook no doubt made many of the observations himself, and certainly took a close interest in the whole procedure. The position of Resolution and the results of the frequent observations are extensively reported in his Journal.
The story of the Arnold watches during this voyage is an unfortunate one. They did not behave well, and throughout the reports of the voyage attention is concentrated on the performance of Kendal’s watch, K1, a replica of Harrison’s No. 4. At the beginning of the voyage, K1 was rated at Greenwich and found to be 7/10 of a second fast on Mean Time and its rate 5/8 of a second per hour slow on Mean Time.

It is typical of Cook that before leaving England he wished to have an exact point of departure and, in order to do so, with the help of his Master, Mr Gilbert, made a survey of Plymouth Sound, in order to decide that his point of departure lay in latitude 50° 19’ N and longitude 4° 23’ W.²⁶

The full set of the observations of the voyage was later published by Wales in his Astronomical Observations. It will be sufficient here to mention a few examples only.

The first one of interest occurred on 29 July 1772 at Funchal, Madeira. It was here in 1764 that William Harrison, on the way to the West Indies, in order to ascertain the functioning of his watch, H4, made the longitude of Funchal to be 17° 10’. On Cook’s voyage, with Kendal’s K1 watch, an identical reading for the longitude was obtained.

Observations on shore were made at the Cape of Good Hope, and later, while the ship lay in Dusky Bay on the southwest coast of New Zealand, where an observatory was set up ashore. This was on 11 May 1773.

The following observations at Dusky Bay are typical:

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<td>— by Mr Kendal’s Watch supposing it to have gone mean time from England</td>
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<td>— Supposing it to have gone mean time from the Cape</td>
<td>164 29 49½</td>
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<td>— Supposing it to have gone at its Cape rate</td>
<td>165 12 36½</td>
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<td>— Supposing it to have gone at its Greenwich rate</td>
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On 21 March 1775 the ship sighted Table Mountain, near Capetown, and from the longitude as determined by the watch the error was found to be no more than 18’ too far to the east. Cook states that the difference found between the longitude by the watch and the lunar observations since New Zealand had seldom exceeded ½°.

At this point in the long voyage both Cook and Wales had come to appreciate the accuracy of the Kendal watch. So much was this so that when Cook departed from Capetown on 26 April 1775, because of his belief in Kendal’s watch, he resolved to try to make the island of St Helena by a direct course. This he did, arriving within sight of the island on 15 May.
Regarded only in terms of progress in navigation, these two voyages covered a period of considerable interest. On the first voyage Cook, a self-taught mathematician and astronomer, adopted the most recent instruments and methods, and practised these throughout the whole of his voyage. It was on this first voyage that he made the most extensive geographical discoveries and himself drew accurate charts of New Zealand and the east coast of Australia. The accuracy of his charts, since most of the observations were made from the ship at sea, was governed by the accuracy of his navigational observations.

By the time he returned to England and embarked on the second voyage, the Board of Longitude agreed that an extensive trial of the use of the chronometer was desirable. The trial that Cook and Wales gave to the Kendal watch during the second voyage could not have been more thorough and extensive.

Although Cook himself made no original contributions to the science of navigation, his voyages proved the theories and methods devised by the mathematicians, the astronomers, and the instrument-makers of Great Britain at that time.

William Wales, who commented in 1788 on the three voyages as a whole, states:

That part of natural knowledge which may be called Nautical Astronomy was undoubtedly in its infancy when these voyages were first undertaken. Both instruments and observers, which deserved the name, were very rare and so late as the year 1710 it was found necessary, in the appendix to 'Mayer's Tables', published by the Board of Longitude, to state facts, in contradiction to the assertions of so celebrated an astronomer as the Abbe de la Caille, that the altitude of the Sun at noon, the easiest and most simple of all observations, could not be taken with certainty to a less quantity than 5, 6, 7 or even 8 minutes. But those who will give themselves the trouble to look into the 'Astronomical Observations' made in Captain Cook's last voyage, will find that there were few, even of the petty officers, who could not observe the distance of the Moon from the Sun or a star, the most delicate of all observations, with sufficient accuracy.27

Cook's attitude, even to the most difficult calculations, is typified by his statement:

Would sea officers once apply themselves to the making and calculating these observations they would not find them so very difficult as they at first imagined, especially with the assistance of the Nautical Almanac and Astronomical Ephemeris, by the help of which the calculations for finding the Longitude takes up but little time than that of an Azimuth for finding a Variation of the compass; but unless this Ephemeris is published for some time to come more than either one or two Years, it never can be of general use in long voyages. . . .28
By the beginning of the nineteenth century the making of chronometers had become a considerable branch of the watch- and clock-making trade. Indeed, from 1800 to 1840 the demand for them far exceeded the supply. The general issue of chronometers to the Royal Navy did not come into force until about the year 1825, and for many years the issue was restricted to one chronometer per ship. Nevertheless, the Astronomer Royal continued to issue in the *Nautical Almanac* the data required for the determination of longitude by lunar distances, and this practice was not discontinued until 1907.

But prejudice died hard. Captain Joshua Slocum, who sailed around the world alone about 1895, stated:

> At Yarmouth, too, I got my famous tin clock, the only timepiece I carried on the whole voyage. The price of it was a dollar and a half, but on account of the face being smashed, the merchant let me have it for a dollar. My tin clock and only timepiece had by this time lost its minute hand, but after I boiled her she told the hours and that was enough on a long stretch.

Gould comments:

> As a feat of singlehanded sailing Captain Slocum’s circumnavigation in the Spray . . . has never been equalled. She is believed to have foundered with her brave master in the course of a subsequent voyage.

**TECHNICAL DESCRIPTION OF ‘LUNAR DISTANCES’**

Throughout Cook’s Journal of the first voyage there are frequent references to the *Endeavour’s* longitude calculated from observations of the Sun and the Moon. This method, which was so well developed for use by practical navigators by Nevil Maskelyne, and thoroughly tested by Cook and his collaborators, is now only of historical interest. It adds greatly to an appreciation of Cook’s skill to recall the accurate observations and, for a seaman, the intricate calculations involved.

Two portions of Cook’s holograph log have survived; one of these finishes with a printed form entitled, ‘Computation for finding the Longitude by Observations taken . . .’ In Cook’s handwriting, this heading is completed ‘12th March 69 from the Sun and Moon’. On this day the *Endeavour* was sailing in the Pacific on a course N 49° W towards the Tuamotu Archipelago, which was sighted first on 4 April 1769.

A reproduction of this form illustrates the task of the navigator. First came the observations. Time by the ship’s watch was recorded, followed by the angular distance between the Sun and Moon measured with the sextant. The altitude of the Sun and of the Moon above the horizon was then measured. Then follows the latitude, calculated from an
Fig. 13. The form filled in by Cook in calculating the longitude on 12 March 1769 (by courtesy of the Trustees of the British Museum, London)
observation of the Sun’s altitude, and the declination given in the *Almanac*. Then the longitude, computed by dead reckoning.

Since the ship’s watch could not be relied upon to keep accurate time, a calculation follows of the exact local time. This is the solution of the spherical triangle on the celestial sphere, of which the three sides are known.

The Sun’s declination is taken from the *Almanac* to give the polar distance of the Sun (PD). The zenith distance (ZD) is obtained from the observed altitude of the Sun, and the triangle is completed by the co-latitude (CoL).

The hour angle (HA), the interval of time before or after noon, is then calculated from the equation:

\[
\cos^2 \frac{1}{2} HA = \frac{\sin S \sin (S - ZD)}{\sin PD \sin CoL}
\]

Where \( S = \frac{1}{2}(ZD + PD + CoL) \)

The printed form enables the navigator to fill in the appropriate values and their logarithms, and to complete the calculation by arithmetic.

Then follows a series of calculations to correct the observations for the semi-diameter of the Sun and of the Moon, the effect of refraction in the atmosphere, and for parallax.

The final step is of interest. Maskelyne’s *Nautical Almanac* records the calculated angular distance between the Sun and Moon at noon and for intervals of every three hours at Greenwich. The navigator compares his observed value with the *Almanac*, and, by simple interpolation, can decide the time at Greenwich of his observation.

In the example of Cook’s observation on 12 March 1769 his corrected observations give the lunar distance as 62° 50' 59".

His rough value for Greenwich time from longitude ‘by account’ is 9 hours 58 minutes. The values for the lunar distance from the Sun taken from the *Almanac* are:

<table>
<thead>
<tr>
<th>Time</th>
<th>Value</th>
</tr>
</thead>
<tbody>
<tr>
<td>9 hours Greenwich</td>
<td>62° 16' 56&quot;</td>
</tr>
<tr>
<td>12 hours Greenwich</td>
<td>63° 56' 54&quot;</td>
</tr>
</tbody>
</table>

Difference 1° 39' 58"

The difference between his observed value and the value for 9 hours at Greenwich is 34' 3". By proportion, this gives the Greenwich time as 1 hour 1 minute 21 seconds after 9 hours, or 10 hours 1 minute 21 seconds.

His corrected local time is 1 hour 42 minutes 44 seconds, and by difference the longitude is 124° 39'.
A distinctly human touch is revealed by an examination of the figures on the form. In the calculation of local time from the Sun’s altitude the logarithms written down are incorrect, and give the wrong answer for the hour angle. The final result is correct; that is, 1 hour 42 minutes 44 seconds. Perhaps a second calculation was made on rough paper.

It is doubtful if, in spite of the obvious attempts made to do so, these calculations could be reduced to a routine. The liability of error in the absence of understanding of the meaning of the many steps is quite great. It is not surprising that the task was regarded as too involved by most seamen. Although Cook had the assistance of Green on the first voyage, and of Wales later, his personal skill as an observer and his knowledge of mathematics and astronomy show clearly in his use of lunar distances.
The great barrier of reefs that lies protecting the Queensland coast still calls for the greatest respect from mariners. Vessels sailing outside the surveyed route can still, like Cook's *Endeavour*, run aground on an uncharted reef. However, although the hydrographic surveys of the Royal Navy and the Royal Australian Navy have so far been concentrated on the coastwise shipping route within the Barrier and on some of the passes through the barrier, it is hoped that in the next decade the entire continental shelf will be surveyed by the Royal Australian Navy on the scale of 1:250,000. Unofficial hydrographic surveys have been made by seismic exploration parties from petroleum companies on both the northern and the southern ends of the reef, but the lines have been run at some distance apart. A photograph from an Apollo spacecraft shows previously unknown groups of reefs outside the Barrier, east of Princess Charlotte Bay. Unpublished aerial photomosaics prepared for petroleum companies also exist, notably those of the Swain Reefs. These show many uncharted reefs. Additional depth data have been contributed by groups from the Universities of Queensland, North Queensland, and Sydney, collecting samples of sediment from the sea floor.

Scientific knowledge of the Great Barrier Reef today is possibly less advanced than its charting, and it is the aim of this contribution to give an outline of what is at present known of the hydrology, bathymetry, form, biology, and geology of the Reef, and, in so doing, to indicate present trends in its scientific investigation, its financial exploitation, and its conservation.

The Great Barrier Reef grows on the continental shelf common to western Papua and Queensland, and is over a thousand miles long. It consists of thousands of separate reefs, many dry or barely awash at low tide, some with coral sand cays, others fringing high islands or the mainland coast. The reefs vary greatly in size and shape. Each is a mass of calcareous skeletal detritus and of skeletons formed by marine organisms, mainly corals, hydrocorallines, coralline algae, molluscs, foraminifera, echinoderms, crustaceans, ascidians, and polyzoa. Corals
and hydrocorallines form most of the 'bricks' of the framework of a reef; coralline algae and some of the polyzoa form 'cement' binding the bricks together; and the others, with the skeletal detritus, fill the interstices of the framework.

Corals are carnivores, living on the small floating or barely motile minute animals and larvac that constitute the zooplankton; the zooplankton lives ultimately on the microscopic floating plants, the phytoplankton, which, like the coralline algae, extract phosphates and other nutrient salts from solution in the sea water and, in the presence of sunlight, synthesise proteins and starches from carbon dioxide and water. Adequate plankton crops, nourished by adequate nutrient salts, are thus essential for reef growth. Reef corals are quite selective; they grow best in shallow, sunlit water between low water mark and six fathoms, but they can still construct reefs in water as deep as twenty-two fathoms, and they may have a sparse existence between twenty-two and thirty fathoms. They prefer water of normal salinity and with an annual maximum temperature above 22°C but below 28°C.

The Great Barrier Reef forms but one of the many reef provinces of the southwest Pacific, all of which are located in the Indo-west Pacific tropical biogeographical province of the continental shelves. It lies on the shallow Queensland-Papuan continental shelf, within the surface water layer, approximately 100 metres thick, of the southwest Pacific Ocean. The only published analytical studies of the physical, chemical, and biological characteristics of this shallow water body over the Reef are those made in the Low Isles region by members of the Great Barrier Reef Expedition (1928-9), and published by the British Museum (Natural History) in Volume II of the Scientific Reports of that expedition.

In that part of the Reef there is little seasonal difference; the temperature of the surface water is high, ranging from 21-24°C to 29-38°C; vertical gradient is small, because of perpetual mixing of waters owing to the action of the trade winds. Average salinity in 1928-9 was about 34.7 parts per thousand, with a maximum about 35.5 parts per thousand; the large reductions caused by land drainage and rain falling on the sea were gradually counterbalanced by more saline waters extending inwards from beyond the Barrier. Humidity normally is high and evaporation small. In 1928-9 oxygen content was high, being over 90 per cent saturation most of the time; in deeper water during calm periods it fell to about 80 per cent. Certain plant nutrient salts were present in extremely small quantities (average 5 mg of phosphate per cubic metre) throughout the year in all layers of water.¹ The supply of nutrient salts in the waters about Low Isles came not from the land but by regeneration from the sea floor. Phytoplankton production,
though small, proceeded with maximum efficiency and little variation throughout the year. Diatoms were predominant over dinoflagellates inside the Barrier, and coccolithophores were never important; the opposite is the case outside the Barrier. The animal plankton, which was comparatively rich, was probably the maximum that the plant crop could support, and was sufficient to support both the coral population, which feeds at night, and the other zooplankton eaters of these seas. Copepods formed on the average 70 per cent of the zooplankton, but the proportional composition varied throughout the year, with sudden increases in various types of larvae. These plankton studies, made by the Yonge expedition, seem to be the only productivity studies made so far on the Reef. Here indeed is a very important field of work.

Collections of water data exist for other intra-barrier regions, some published, others, by Maxwell et al., 1960-8, unpublished, but have not yet been analysed; it is possible that regional variation inside the Barrier will not be very great.

An investigation of the residual surface drift at the southern end of the Reef has been carried out by Woodhead, who, from drifters released during the winter, has interpreted (personal communication) the drift patterns as generated largely from the interaction of the East Australian Current with the topographic features. In Hervey Bay a large clockwise eddy was generated from this current by the obtrusion of Fraser Island and Breaksea Spit. Another large eddy was formed from the main southerly flow as it passed the Swain Reefs; this eddy tended to turn into the Capricorn Channel, through which there appeared to be a northwesterly current, with vague lateral counter-currents. Inside the Capricorn and Bunker groups of reefs the general movement was to the south and west, on to the coast.

Off the seaward edge of the Barrier in the Low Isles region, the top 50 metres were, in 1928-9, essentially similar to the waters inside the Barrier. The only important difference was that a somewhat higher salinity prevailed outside the Barrier, particularly during the rainy season. Between 50 and 100 metres below the surface an abrupt change took place in almost all the conditions. The temperature fell, the salinity rose, pH value and oxygen saturation fell, and phosphate and nitrate rose. The changes in all factors except salinity continued to the greatest depth sampled (600 metres); salinity showed a decrease with depth below 100 or 200 metres.

No clear conclusions on the origins of the Reef waters can be drawn at present, except that they are complex. From studies of the surface circulation in the Coral Sea, it appears that probable surface sources are the surface waters of the Trade Wind Drift, those of the Southern Equatorial and East Australian Current systems, and, in some seasons,
those of the Arafura Sea and the Gulf of Papua, to which may be added monsoonal rain and land-surface run-off.

Almost certainly, also, there are contributions to the Reef water from the subsurface water masses and types of the Coral Sea, by upwelling at the continental slope, and by tidal action. The Coral Sea work of the CSIRO Department of Fisheries and Oceanography is elucidating the stratification of the waters of this part of the southwest Pacific Ocean, and is delimiting the various water masses and water types therein, their physical and chemical characteristics, and their circulation. It should not be impossible in the future to identify such contributions and to know whether any of them promote or deter reef growth. On the shelf, reefs grow more profusely in some situations than in others. Other factors being equal, coral reef growth will be encouraged in those regions where the waters are rich in nutrients and plankton. Such enrichment may well be provided and localised by upwelling currents.

Tides play an important part in determining the positions and shapes of the reefs on the shelf. The Reef tides are diurnal; the tidal range is nowhere less than eight feet, and in two regions it is very great. At the entrance to Torres Strait it is over thirteen feet. In the region between Gladstone and Bowen the range reaches thirty feet in Broad Sound; in this region, it has been calculated that there is a 15 per cent increase (1,000 cubic miles) in the volume of the water over the shelf, and all of this must enter and leave the region twice each day; strong tidal currents result, and their influence on reef growth is significant, especially in areas that are normally protected from oceanic swell. Growth is frequently very great on either side of tidal channels, and the reefs of the shelf edge, if they do not front the oceanic swell, tend to front the co-tidal lines.

The Queensland continental shelf is shallow throughout, the change of gradient between shelf and slope being on the average at 50 fathoms. In the northern region, that is to just south of Cooktown, it is covered with mainly less than 20 fathoms of water. In this region the shelf is quite wide east of Cape York, but narrows gradually as the coastline trends south and east to Cape Melville, where it is only 15 miles wide. This section of the shelf is bounded by the Papuan and Queensland Trenches. In the central region, and south to about Mackay, the shelf widens again and the water in its wider outer part deepens to mainly less than 30 fathoms. In the southern region the widening and outward deepening continues to the Swain Reefs, which rise through water of average depth 32 fathoms. Outside this part of the shelf the Coral Sea Platform rises from the Coral Sea. The shelf narrows again south of the Swain Reefs and around the Curtis Channel, and from Gladstone southward is again mostly covered with less than 20 fathoms of water. Outside
this part there is a very gentle slope from the shelf to the abyssal regions of the northern part of the Tasman Sea.

Platform-like surfaces and changes of gradient are distinguishable at several levels on the shelf: at 10, 16, 20-22, and 32 fathoms, and less certainly at 36, 56 and 80 fathoms. These may be related to periods of still-stand in sea-level, during the fluctuations associated with the Pleistocene glaciation. This would imply that there has been no post-Tertiary cross-warping of our coastline. Maxwell interprets the 32-fathom platform as formed during a regressive phase of the sea, and, like some earlier workers, considers that a bench 6 to 10 ft above present sea-level was formed during the latest post-glacial maximum.\(^\text{11}\)

The topography of the shelf, when considered together with the sediments deposited on the shelf and their thickness as indicated by some few traverses with an echo-sounder, suggests that valleys that owe their sculpture to ancient sub-aerial erosion cross the shelf from the present mouths of rivers to some of the deeper passes in the outer barrier. A detailed bathymetric survey made off Townsville in the vicinity of Magnetic and Palm Passages shows that the pre-Holocene fall of sea-level almost completely exposed the reef then (W. Sugden, personal communication, 1969).

Consideration of the types and positions of the reefs, and of the sediments being deposited on the shelf, permits a division of the province into zones sub-parallel to the shore. There is a near-shore zone (down to 5 fathoms), within which reefs are relatively few but may be found on rocky headlands, on long straight stretches of the coast adjacent to relatively deeper water, and near the mouths of rivers. An inner shelf zone is delineated by the 5- and 20-fathom contours, and forms a band about 23 miles wide of relatively even topography and uniform gradient. Its reefs, like some of those of the near-shore zone, appear not to have been completely planed off to sea-level since the recent 6-10 ft emergence. The outer (or marginal) shelf is the zone between the inner shelf and the shelf edge, that is between the 20- and 50-fathom contours. It is not recognisable in the northern region, but appears in the central region and widens progressively southwards to include the Swain Reefs, where it is embayed from the southeast by the Curtis Channel. West and southwest of the Curtis Channel it is again very narrow, but supports the Bunker and Capricorn groups of reefs. In the relatively unprotected areas of the outer shelf all the reefs are ‘clean’; if any of them did reach the 6-10 ft maximum sea-level, they have all subsequently been planed off to the present sea-level, and are now broad robust structures actively growing at the edges. They contrast with the frequently embayed, ‘corrugated’ relics of the near-shore and inner-shelf zones of the northern region.\(^\text{12}\)
Reefs grow actively outward as well as upward. Any one reef, having depth and temperature fixed by its location, will have its shape determined by the direction and force of the water currents bringing the zooplankton and by the shape of the base on which it grows. Where the forces of growth are equal in all directions, radial expansion results in platform-type reefs, such as are found on the outer shelf behind the main line of the shelf-edge reefs. With further radial growth, lagoonal platform reefs develop. When bathymetry varies, for instance if the reef grows on a sandbank, elongation may result, but the elongate platform does not develop a true lagoon. Where the direction and force of currents and the bathymetry are markedly asymmetric, reef growth is restricted to one or two directions, resulting in various types of linear reefs, such as the wall reef, and the cuspate reef with the ends curving leeward; or alternatively, the prong reef develops in response to high leeward turbulence.

The ‘seaward’ edges of the Queensland reefs, that is the edges fronting the prevailing swell or the tidal progress, and the edges fronting tidal channels, practically all have a raised algal (Lithothamnion) ridge that is just above low tide, and seaward of this, in the upper two fathoms of slope, a system of spurs and grooves or surge channels at right angles to the front. The reef expands by the active forward growth of the spur ends. Such an arrangement clearly exposes the maximum amount of growing coral to the currents while the binding action of the Lithothamnion gives great rigidity. The purplish-red colour of the Lithothamnion ridge is very distinctive. In the lee of the Lithothamnion ridge of asymmetric reefs, ‘foul ground’ develops when isolated patches occur of more rapid upward growth of coral. Similar patches occur in the lagoons of platform reefs; some platform reefs may be entirely surrounded by an algal rim. It is notable that many of the westernmost reefs of the outer shelf present an algal rim and a radially grooved upper slope to the west and south, probably mainly in response to the size of the waves generated by westerly and southerly winds, but also in response to refraction of the wave fronts of the prevailing southeast swell and to strong tidal currents.

Zonation is a notable feature of the reefs. The relatively infertile, ripple-marked off-reef or inter-reef floor may be separated from the reef slope by a ridge, possible a scree, and corresponding channel. The slope, which averages 30°, increases towards the surface, and is interrupted by terraces of varying width covered by sand and gravel and supporting a reduced reef fauna; the most persistent terraces are at ten fathoms, seven fathoms, and two to two and a half fathoms. At the top two fathoms of the slope is the reef front—and this is the part that is characterised by spur and groove structure, and is the zone of most active coral growth. Then follows the low algal rim, which may be up
to five hundred yards wide, and is bare at low tide. Behind the algal rim is the reef flat, mainly bare at low tide, which is divisible into three zones, first the zone of living coral and coral pools, then a dead coral zone, and then the sand flat, all three zones having a marked radial pattern, reflecting the direction of wave translation across and tidal withdrawal from the reef. Depending on the type of reef, as one proceeds further across the flat one may encounter either a sand cay or the dead coral zone of a leeward flat, or a lagoon, up to six fathoms or even twelve fathoms deep, or the back-reef apron and foul ground that falls away to the leeward off-reef floor.

Cays originate from sediment lifted from the reef surface and carried forward by converging tidal currents or converging transitory waves until water velocity is reduced abruptly in the area of convergence. Wind action at low tide on these deposits may build dunes above high-tide mark. Vegetation or the development of beach rock may stabilise such cays. Storm-wave attack has formed shingle banks and boulder tracts. Boulder tracts are common on the island reefs of the near-shore and inner zones, and on these reefs possibly represent residuals from an older, higher reef surface. Shingle banks are widespread, and dark-coloured banks formed at earlier levels may lie side by side with white banks presently forming.15

Marine algae are second in importance as reef organisms only to the corals. Ecological studies of Low Isles and of Heron Island16 give good bases for the comparison of algal zonation on these two reef types. As mentioned above, the encrusting red algae Lithothamnion and Porolithon form the purplish-red algal rim that is so characteristic a feature of the Great Barrier Reef. Green algae play only a minor role in fortifying the Reef against wave attack, but they are important in their contribution to sediment. Halimeda flourishes in almost every part of the Reef: middle and outer reef flat, algal rim, reef front, reef slope, and lagoon. Acetabularia is restricted to the reef flat, and Penicillus occurs only on the inner-reef flat.17

Apart from the marine algae of the benthos and the phytoplankton, the plants of the Reef are those of its coral cays and of the raised reef surfaces of the inner-reef belt. The botany of the high islands, such as the Whitsunday Group, is not considered here, since it is the same as that of the adjacent mainland. The terrestrial flora of the cays is a very restricted one, of only thirty to forty species, practically all of which are of wide distribution in the Indo-west Pacific province, and are characteristic of strandline environments and of environments of shifting lime sand, in some cases modified by bird guano.18 Perhaps the most interesting distribution of plants within the Reef is that of the two mangroves, Rhizophora and Avicennia. They occur on the cays only as far south as the
Low Isles, though they are common on mainland mudflats the full length of the Queensland coast.\textsuperscript{19}

The marine fauna of the Reef is now shown to form a special Great Barrier Reef or Solanderian subprovince of the Indo-west Pacific tropical shelf province. Studies on the intertidal faunas of the Queensland mainland, and on those of the continental islands and coral cays, suggest that the narrow zone of overlap between the reefal fauna and the normal Australian tropical shelf fauna lies outside the continental islands, but between the inner reefs and the mainland. Similar conclusions are reached from a study of the echinoderm fauna (excluding crinoids), although the serpulids of at least some continental islands are more similar to the serpulid fauna of Heron Island than to that of the mainland, and no difference has been found between the simple ascidian fauna of the Reef and that of the mainland.\textsuperscript{20}

The Reef fauna is a very rich one, with, for example, 350 species of coral and 146 species of echinoderms. Both coral fauna and echinoderm fauna\textsuperscript{21} suffer reduction in number of species southward as the temperate shelf waters are approached. Of the 94 species of purely Reef echinoderms, 15 are endemic to eastern Australia.

Currents permit west Pacific species to migrate as pelagic young to the Great Barrier Reef, but there can be no movement in the reverse direction.\textsuperscript{22} It seems quite possible that a reef the coral of which has been decimated or even extinguished by exceptional predation, such as by the crown of thorns starfish (\textit{Acanthaster planci}) which has reached plague proportions and is known to have eaten out much of the living coral from the reefs opposite the southern end of the Queensland Trench, may be repopulated by coral larvae carried in from the Coral Sea Platform. The cause of this plague may well be the removal by tourists and shell collectors of the giant tritons and other shellfish that are natural predators of the crown of thorns. If natural repopulation by coral larvae does not occur, possibly the plague might be reduced by artificially increasing the giant triton population.

Endemic Great Barrier Reef species may have originated at a time during the Pleistocene ice age, when sea-level stood so low that Cape York was joined to Papua by land.\textsuperscript{23}

Taxonomic evaluation of the Great Barrier Reef fauna was rather slow until the Murray Island Expedition of 1913 and the Great Barrier Reef (Yonge) Expedition of 1928-9 began to publish results, and until the number of Australian universities and the numbers of the zoological staffs of the universities began to increase after World War II. At about this time, too, the Heron Island Research Station was originated (as the Heron Island Marine Biological Station) by the Great Barrier Reef Committee, a body constituted in Brisbane in 1922 of those individuals
and institutions interested in the development of the scientific knowledge of the Reef. This Committee supplied much of the drive responsible for the Yonge Expedition, and has subsequently put down two borings into the Reef and supported much other research. There is by now some continuity in universities in Queensland and New South Wales in taxonomic studies of the Reef fauna, and some of the State Museums maintain named collections.

The numerous taxonomic papers in Volumes IV to VII, published by the British Museum (Natural History), on the work done by the Yonge Expedition, are fundamental to all subsequent work, but perhaps even more significant are the trend-setting papers in Volumes I to III, on the physiology and biology of reef-building corals, on the ecology of coral reefs, and on the physical and chemical characters and the plankton of the reef waters. As the contribution of a single reef expedition, the whole record remains unsurpassed.

The collections made by the Murray Island Expedition were from the northern region, those made by the Yonge Expedition were from the Low Isles (Port Douglas and Cooktown) region, whilst much of the work of the universities has been based upon Heron Island and concentrated on the southern end of the Reef.

More or less complete monographic studies on Great Barrier Reef animals are now available from all the above sources combined; the following are perhaps the most useful: foraminifers, sponges, hydroids, siphonophores, alcyonarians, anemones, corals, worms, polyzoa, lamellibranchs, gastropods, echinoderms, lobsters, crayfishes, prawns and crabs, tunicates, enteropneusts, fishes and birds.

Zoo- and phyto-plankton studies are those of the Yonge Expedition published in Volumes II, IV, V, and VI of the reports, with a few smaller recent additions.

The Reef has so far contributed little to our maritime industries. The irregularities of the sea floor around the reefs preclude extensive net fishing there. In the reef-fishery proper, only about twenty-five species are fished. Most demersal reef fish are believed to spend their entire adult life in the vicinity of the reefs they inhabit, but their spawning behaviour and general biology are not well known. A small amount of trolling for mackerel and coral trout is done in Reef waters, mainly against the reefs. Spanish mackerel congregate at the southern limits of the Reef in May and June and migrate northwards. In August there are great concentrations east of Bowen, and by October they are east of Townsville. When the shoals are between Townsville and Cairns, spawning occurs, after which the fish return southwards. Tuna is taken in the Coral Sea outside the Reef, mainly by Japanese and Taiwanese. The taking of turtles is now prohibited.
Possibly, in the future, ways will be found to exploit the immense harvest of 'trash' fishes so as to make a proteinous concentrate, but it will be necessary to separate the toxic fishes. First, much needs to be learned of the life cycle of fishes. Biomass studies also are urgently required.

Large beds of prawns have been discovered in the sea floor in 9 to 18 fathoms off Townsville and Cairns, and trawling has begun. The deadly sea wasps, the jellyfish *Chironex fleckeri* Southcott, appear in the waters above the prawn beds and apparently feed on the prawns (R. Endean, personal communication). Lobsters (painted crays) may prove exploitable.

Beds of saucer scallops have been located in 10 fathoms in the near-shore zone off Rockhampton and Bundaberg. The trochus industry, like the edible beche-de-mer industry, is now defunct, but the pearl-shell industry is still important in the economics of the Torres Strait islands. Though world demand for mother-of-pearl has been reduced by increased use of plastics, live shells are still gathered in considerable numbers, but are taken to culture farms for the making of artificial pearls.

Officers of the Division of Fisheries and Oceanography of the Commonwealth Scientific and Industrial Research Organisation have published studies on the taxonomy and reproduction of oysters and pearl oysters.

Another possibility for future exploitation of the reef exists in pharmacology. The toxin studies so far made are directed mainly towards the identification of the toxin, the manner in which it affects victims, how it affects isolated nerves and muscles, and the preparation of an antidote. Some marine creatures inject their toxin into their victims; these include the sea wasp, the stone fish, and the gastropod *Conus*. There are a number of fishes whose tissues are toxic, some periodically, after they have feasted on toxic algae or flagellates. Many of these toxins, because of their effect on nerves and muscles, have important applications in medicine. Some therapeutic drugs have already been recognised, and some antibiotics have been isolated from the marine organisms that produce them. Physiological and biochemical studies of reef organisms are urgently required as background for their pharmacology.

Three other aspects of the Reef are exploitable. These are its tourist value, its petroleum and natural gas, and its lime and quartz sands.

The Queensland government is well aware of the potential of the Reef. The report of the Interdepartmental Committee on Leasing and Development of Queensland Islands (1966) made restrained and sensible recommendations for realising the tourist potential. The committee
recognised that the number of habitable coral islands is limited and also that tourists, if not controlled, rapidly despoil the beauties they seek. It is proposed that a system of national parks and scenic areas be integrated with leases for tourist resorts so that such control can be exercised, and conservation ensured, without at the same time preventing the exploitation of the reefs by industries other than tourism. A report by Harry S. Ladd, of the U.S. Geological Survey, to the Queensland Minister for Mines advised that the Reef was large enough to accommodate easily the scientific and economic interests of all, including the petroleum and lime sand industries, but that scientific studies of the likely effects of the exploitation should be undertaken before sites were chosen. A report on the ways of preventing a Santa Barbara type of disaster from oil is currently awaited by the Queensland Minister for Mines.

The off-shore petroleum industry has already shown in southern waters what riches it can bring to Victoria, and it seems not improbable that a reef-based petroleum industry could be established in Queensland. Large mineral and oil prospecting companies commonly foster basic research in relation to their areas of interest; it could be expected that a reef-based petroleum industry would finance zoological and ecological as well as geological research. Several petroleum companies have already paid out large sums on geophysical surveys of the Reef. Three offshore wells have been drilled, all, unfortunately, dry holes. A fourth is now drilling at Anchor Cay, to the northeast of Cape York. Two earlier holes were drilled by the Great Barrier Reef Committee.

Geophysics, a very helpful but very costly tool, is contributing a great deal to our understanding of what is below the Reef, and hence to our study of its origin. Financed mainly by petroleum exploration companies using both aerial and marine surveys, but in part subsidised by the Commonwealth government, geophysicists have produced maps interpreting the subsurface geological structure. Under the Petroleum Search Subsidy Act 1959-1964, information obtained from subsidised operations may be published by the Commonwealth government six months after the completion of the field work, and this provision has proved most beneficial to our knowledge of the Reef. Initial minor gravity surveys were made by the Bureau of Mineral Resources, and some minor seismic work has been done by the Bureau and by the University of Queensland.

Three main geophysical approaches have been used—gravity, magnetic, and seismic. In each method it is anomalies—departures from an expected pattern—that form the basis of the interpretation maps. If the material below the earth's surface were of uniform composition, and hence of uniform density, the value of gravity over the earth's
surface would be constant, that is there would be no gravity anomalies. In practice, anomalies are always encountered, and can be interpreted in terms of broad density (hence rock) variation.

Similarly, deviations from the expected magnetic picture can be interpreted in terms of depth to 'basement' rocks, which are normally more magnetic. Sedimentary rocks that could contain petroleum have low magnetism, and normally lie as 'cover' rocks over 'basement'.

Seismic methods give by far the most detailed and helpful geophysical picture of the subsurface geological structure, but they are also the most expensive. Sound waves are generated at successive observation points, and reflected back from surfaces bounding rock layers of different density. By controlling the source, and the pattern of reception of the sound waves, one can make a map showing the depth of these 'reflectors' and their topography. From their topography, their density, and their thickness, the geologist can select the more promising places to drill for hydrocarbons or minerals. He can even point to certain anomalies as being possibly reefal in origin. Association of petroleum reservoirs with reefs is common elsewhere, and could well be the case here also.

Less than one-third of the Reef has been surveyed geophysically so far, but already some areas are indicated where a sufficient thickness of sedimentary rocks exists under the reef to warrant the drilling of wells. Conversely, geophysics has been useful in indicating rather quickly areas in which further expenditure is not warranted at present. But considering the geophysical evidence and the evidence of the few holes drilled so far, we see that we have not nearly enough data for a definitive geological history of the shelf on which the Reef stands, nor can we yet outline the history of the Reef itself. Without a knowledge of the geology of the Reef no exploitation program can be planned or controlled.

Current petroleum interest centres on the considerable thickness of Mesozoic and Cainozoic strata indicated under the shelf in the northern region between Papua and Princess Charlotte Bay by geophysical surveys. The indications are that towards the edge of the shelf in the north, Palaeozoic basement may lie at depths ranging from 4,000 to as much as 10,000 ft, and that somewhat folded Mesozoic sediments overlie the basement and are transected along or near the shelf edge by a NNW system of faults that may represent the continuation of the Komewu Fault of western Papua.

The Komewu Fault is a pre-Tertiary fault marking the hinge between the western stable platform and a gently folded eastern belt. Subsequently it may also have marked the eastern front of the shelf-type Miocene Barrier Reef of Papua though lower Miocene reefs also formed on shoal areas in front of the main reef platform. A little-disturbed blanket of Cainozoic strata overlies the Mesozoic with erosional
unconformity, and by analogy with Papuan geology it may be that Miocene strata will be included and will consist of shallow water limestone with local reef developments.

The hole currently drilling in Anchor Cay, 120 miles northeast of Cape York, will shortly provide factual knowledge of the sediments below the shelf here. The Quaternary section includes coral reefs and associated calcarenites, with basic volcanics. Present-day reefs appear from the geophysical results to be sited above areas of pre-Tertiary anticlines and upfaults.

No great thickness of sediments above basement is indicated beneath the inner shelf opposite Cooktown and Cairns by gravity survey; nor is the existence of any faults of large vertical throw.

Off Townsville, in Cleveland Bay, a seismic (sparker) line has disclosed an upper sedimentary sequence of about 350 ft that thickens seawards regularly to more than 1,000 ft at the landward entrance to Magnetic Passage (W. Sugden, personal communication). This may be Cainozoic, conceivably Miocene to Recent.

A downfaulted trough, the Hillsborough Basin, nine miles wide, with a maximum of 10,000 ft of Tertiary (and possibly late Cretaceous) sediments, mostly non-marine, has been delineated, beginning inshore at Proserpine and thence running just off and parallel to the coast southwards towards and perhaps (from sparker survey) into Broad Sound. No marine Miocene has been identified.

Under the southern part of the Reef there appears to be a subsurface ridge, the Bunker High, of Lower Cretaceous Graham's Creek Volcanics, running from the northern part of Fraser Island under the Bunker and Capricorn groups of reefs, a ridge over which the Wreck Island bore disclosed some 835 ft of marine Miocene sediments followed by some 345 ft of Pliocene, and then by the top 615 ft in the bore which is considered Pleistocene and Recent. The boundary between Pliocene and Pleistocene is at present drawn at 682 ft below the surface of the Heron Island bore, on the basis of the occurrence there of similar species of foraminifers to those used rather arbitrarily for drawing this boundary in the Wreck Island bores. The Tertiary marine sediment intersected beneath Wreck Island is considered to be carbonate rock, formed in a reef province.

The Bunker High appears to form an eastern structural boundary to the marine Cretaceous Maryborough Basin, which also appears to be closed to the north some sixty miles north of Bundaberg. Between the Bunker High and a second, parallel High (Swains High), which runs NNW and extends under the Swain Reefs, is a basin (the Capricorn Basin) with some 8,000 ft of sediment, apparently thickening eastwards opposite Rockhampton. Two borings have tested this sequence.
the eastern hole (Aquarius) encountered at about 8,670 ft basement of low-grade Palaeozoic? metamorphics, on which lie some 3,100 ft of non-marine, possibly Mesozoic claystone, some 4,550 ft of Tertiary and some 785 ft of Quaternary sediments. About 995 ft of the Lower Tertiary is non-marine; the rest of the Tertiary comprises marine marls and limestones, the Miocene sequence being 2,086 ft thick and the Pliocene 969 ft. How much of the Tertiary marine strata is reef rock is uncertain; the top 27 ft of the Aquarius hole was described as Recent reef rock, and corals and coralline fragments were frequently encountered in the boring.

From these very sparse observations it appears not improbable that much of the present shelf was covered by the sea during the Miocene and perhaps also the Pliocene, and that the region was a reef province then, as now. It was uplifted above sea-level, at least in places, during the lower Tertiary, and probably most of it was exposed to sub-aerial erosion at some time during the sea-level movements of the Pleistocene. Only an arbitrary bottom limit can at present be assigned to the Pleistocene, based on the ranges of three species of foraminifers, and any boundary drawn between Pleistocene and Recent would be even more arbitrary. Faulting parallel to the present coastline has certainly occurred, and the fault troughs were filled with Tertiary sediment, much of it terrigenous, but the age of the faulting is known only within a wide range—post-Lower Cretaceous to Lower Tertiary. It is probable that there has been subsequent movement along some of these planes.

The sediments of the Great Barrier Reef Province have been the subject of nine years' study by W. G. H. Maxwell and his collaborators, culminating in his *Atlas of the Great Barrier Reef*. He has found that the sediments now being deposited in the near-shore zone are terrigenous and include, on the beaches, fine quartz sand, coarse quartz-feldspar sand, and gravel; and on the tidal flats, mud and sand. On the inner shelf terrigenous muds and sands are being deposited, but there are localised carbonate concentrations.

The quartz and quartz-feldspathic sands and gravels are derived from the large dune systems of the present coastal region, from the buried dune systems of earlier sunken coastal regions, and from the granites of the hinterland and islands. The non-carbonate muds average kaolinite 20 per cent, montmorillonite 60 per cent, and illite 20 per cent, and there is no marked progressive change in the proportions of these clay minerals with distance from shore.

The outer or marginal shelf, where maximum reef growth occurs, is covered by fine muddy carbonate sands. On the reef surfaces, carbonate gravels and coarse sands are dominant on the reef flat, and finer sands and silts in the lagoons, while boulders and shingle banks may occur
on the reef crests or rims. The major components of these reef surface sediments are lithothamnioid algae (17-40 per cent), green algae *Halimeda* (10-30 per cent), corals (20-40 per cent), foraminifers (8-20 per cent), molluscs (4-15 per cent) and echinoid, bryozoan, and crustacean debris (together up to 5 per cent). In the inter-reef areas the relative proportions change markedly—lithothamnioid algae (0-15 per cent), *Halimeda* (5-65 per cent), corals (5-10 per cent), foraminifers (15-40 per cent), molluscs (20-35 per cent), and bryozoans (5-30 per cent). In addition to the detritus from the present reef faunas, a large proportion is derived through the erosion of old, dead reef surfaces left emergent after the last fall in sea-level. It can be identified by its degree of alteration, and dated by carbon isotope studies.

**Facies** changes in the sediment are abrupt, and the factors responsible for the distribution pattern have been examined in detail. Maxwell considers that the total volume of sediment being supplied from the granitic and sandstone land surface, from the coastal dunes and from the drowned dunes, and from the skeletons of the reef animals, is at present very small, owing to the latest emergence of 6-10 ft not having had any reversing effect on the previous submergence. When the sea was at its lowest level, at the end of the Tertiary epoch, the land mass was reduced by active erosion, stream beds were deeply incised, and considerable quantities of sediment were carried towards the shelf edge. As the sea advanced over the land, base-level was raised, erosion became less effective, and much of the stream load was deposited in the previously deepened valleys. With further transgression, existing reefs grew surface-ward and were not eroded until they reached the surf zone. Thus the transgressive phase encouraged reef growth but reduced the scale of deposition. Temporary regression of the sea, giving emergence of the land, could reverse this trend by lowering base-level, but the latest emergence seems not to have been sufficient to reverse the effects of the main transgressive phase, and today there is a state of near equilibrium. We still have old strandline and near-shore deposits submerged and encroached upon by more slowly accumulating finer sediments of mainly terrigenous origin. Carbonate facies have been superimposed on these in areas where reef density is sufficient. The comparatively abrupt transitions between the carbonate, the muddy terrigenous, and the sandy terrigenous facies are considered to be evidence of a low rate of movement of sediment and of a generally low rate of deposition. It seems that negligible sedimentation is occurring on the shelf today, and that sedimentation has indeed been extremely slow since the lowest stand of sea-level after the Tertiary, and especially since the time of the 32-fathom sea-level. Actual thicknesses, however, can only be known after much more geophysical work and drilling. It will not be until we
know the thicknesses and types of sediment deposited during the Tertiary and the Pleistocene and Recent that we shall be able to write the full history of the growth of the Great Barrier Reef.

Negligible economic use has been made so far of the sediments of the reefs. Tremendous resources of lime exist, and there is, off Cooktown, a possibility of tin-bearing mineral sands, but of recent years no licences to mine or quarry the reefs have been issued by the government of Queensland. There are, however, so many reefs available, that sites could readily be selected where the temporary pollution by lime dust caused by quarrying would be localised, and the dusted region re-colonised. Recent excavation of a boat channel on Heron Island showed that re-colonisation can be fairly rapid, though so far not total; re-colonising larvae from the growing sides of the quarried reef, or from reefs close to windward, are supplied by nature. While quarrying is not permitted, an important source of information on reef structure is denied us; three-dimensional studies possible in excavations are far more useful than the two-dimensional ones at present being made.

Conservation as well as exploitation of the Great Barrier Reef is dependent upon legal title; and international agreements and Commonwealth and state laws are all involved. Codification is necessary and clarification of the complex legal situation is now proceeding.

Our survey of what we have found out about the Reef in the 200 years since Cook first discovered it is now concluded. It remains to consider the future.

Four present items of major importance may be cited.

The first is the interest being shown by the petroleum industry in finding out all it can about the geological history of the Reef. The finance and energy being devoted to this deserve the admiration and support of all Australians, scientists and laymen alike, as do the Petroleum Search Subsidy Acts that ensure that the knowledge so gained becomes available to all of us. Subsurface knowledge is very costly to get; without subsurface knowledge we shall never fully understand the origin and growth of the Great Barrier Reef, nor be able to influence its future growth or regeneration.

The second is the agreement now being negotiated, by which the Great Barrier Reef Committee and the University of Queensland will become joint owners and operators of the Heron Island Research Station. This we confidently expect will greatly aid the flow of financial support to the Station and increase our knowledge of the marine biology of the Reef. The volume of research generated from the Station is even now quite large.

The third is the development of new facilities in marine biology and geology in Queensland. Thus a new Department of Marine Biology at
the University College of Townsville has been formed under the vigorous direction of Professor C. Burdon-Jones, who has already initiated a lively and opportune program of research. Courses in Marine Geology will be offered at the University of Queensland in Brisbane from 1970, and a marine geological section is under consideration for the Geological Survey of Queensland.

The fourth is the intention of the Royal Society of London to send a Great Barrier Reef expedition to Pickersgill Reef in 1972; with the immense achievement of the Yonge Expedition of 1928-9 to be emulated, much is hoped of this new expedition.

Truly the present is a time of great promise, but in planning for the future we must see that encouragement is given to all who seek to extend scientific knowledge of the Reef. Obviously we need to recognise our Reef heritage as a talent to be put to use, as in the biblical parable, not as something simply to be cherished. We must get to know its potential, and so organise our use of it that it remains a continuing asset. The Commonwealth and state governments are aware of their responsibilities, and have shown their interest in discharging them to the nation’s best advantage, but there is a need for the enlightened integration of all efforts to learn more of the Reef, and to develop its geological and its biological assets and its beauties. What better body could there be, to provide balanced advice to government, than the Australian Academy of Science?
A botanist and geneticist like myself, in dealing with his scientific problems, has always to be conscious of the history behind the organisms or the communities of organisms which he studies. He knows, as I do, that he will not completely understand the 'how?' and the 'why?' until he knows the 'when?'. To take three examples, all of which I shall mention later: How long have the southern beeches, species of the genus *Nothofagus*, been in the southern tips of South America and Australia, and on the mountains of New Guinea? Or how long has a small buttercup, *Ranunculus acaulis*, been distributed into Tierra del Fuego, Tasmania, and New Zealand? Lastly, how long has the family Proteaceae, the *Banksias*, *Grevilleas*, *Persoonias*, etc., so characteristic of our Australian bush, been in Australia, with three outlying species in New Zealand, a few in South America, and several endemic genera in South Africa? If we knew these times, we could, perhaps, hazard a guess that part of the present distribution of the Proteaceae is a relic of Gondwanaland, and that today’s disjunct distribution of the family into South Africa and Australia is a result of the drifting of the continents apart during Mesozoic times. We might also be able to conclude that the present distribution of the southern beeches is the result of widespread extermination over large areas; for example over the Antarctic continent and much of southern Australia. Any invocation of continental drift as a major part of the explanation of this distribution might be ruled out if we had an accurate knowledge of the time of origin of the genus. Lastly, as regards the species, *Ranunculus acaulis*, again, how old is it? A few million years old? Or a few tens of millions of years? Or a few hundred million years? If it is a young species, clearly it must have got where it now grows naturally by travelling over wide stretches of the southern oceans in the gut, or stuck on the feathers, of some wandering sea-bird, or travelling, perhaps, embedded in a log of wood driven round the southern end of the earth by the Roaring Forties.

We cannot answer any of these difficult questions with any precision at the present time. But I hope to show that there are a few 'straws in
the wind’, or even logs in the sea and fossil grains in the rocks, which show that these problems of ‘when?’ are not insoluble.

It seems to me peculiarly appropriate that someone in this Cook Bicentenary Symposium should attempt to summarise these biological and historical issues. Cook’s first voyage is ‘historic’ in several ways: he developed and demonstrated the new precision possible in navigation of ships at sea; he made geographical discoveries, including Botany Bay, which I can see from the windows of my University office; and lastly, his voyage set the precedent for taking scientists, particularly naturalists, on voyages of geographical discovery. The British Admiralty magnificently responded to this precedent, sending the Forsters on Cook’s second voyage in the Resolution; David Nelson on the third voyage, also in the Resolution; David Nelson and James Wiles in the Bounty and the Providence on Bligh’s two bread-fruit voyages; Menzies with Vancouver in the Discovery; and Robert Brown with Flinders in the Investigator. The tradition continued with Charles Darwin in H.M.S. Beagle under the command of Captain Fitzroy, Joseph Dalton Hooker with Ross in the Erebus and Terror, and Thomas Henry Huxley with Owen Stanley in the Rattlesnake. From the work of all these naturalists grew up an immense body of new data, illustrating first, the apparently endless variety of living creatures existing on this earth today, and second, an appreciation of the strange and unexpected relationships of the floras and faunas of the southern tips of the continents, now separated by thousands of miles of storm-tossed ocean. Darwin, of course, with his insight, was the first to attempt to add the dimension of time in his studies of palaeontology, particularly in South America. In a characteristic passage, he says: ‘The extinction of species has been involved in the most gratuitous mystery’. I might add it still is. Darwin goes on:

Some authors have even supposed that, as the individual has a definite length of life, so have species a definite duration. No one can have marvelled more than I have done at the extinction of species. When I found in La Plata the tooth of a horse embedded with the remains of Mastodon, Megatherium, Toxodon, and other extinct monsters, which all co-existed with still living shells at a very late geological period, I was filled with astonishment; for, seeing that the horse, since its introduction by the Spaniards into South America, has run wild over the whole country and has increased in numbers at an unparalleled rate, I asked myself what could so recently have exterminated the former horse under conditions of life apparently so favourable. But my astonishment was groundless. Professor Owen soon perceived that the tooth, though so like that of the existing horse, belonged to an extinct species.

Darwin’s amazement was akin to Thomas Jefferson’s, when, after a journey into the frontier land of Kentucky to the Big Bone Lick
discovered in the days of Daniel Boone, Jefferson realised that the objects the frontiersmen were using as stools were the vertebrae of an elephant (Mastodon). Jefferson speculated whether the mastodon still existed in North America. He went on to become President of the United States of America and set aside a room in the White House to display his fossils. During the course of his presidency he dispatched Lewis and Clark on their epic journey across North America to the Pacific coast. Whether he hoped they would find living mastodons, I don’t know. Darwin, on the other hand, retired to Down House in Kent, and, twenty-five years later, changed men’s ways of thinking about themselves by writing On the Origin of Species.

It was from the three voyages of the Beagle, the Erebus and Terror, and the Rattlesnake, and from their naturalists, Darwin, Hooker, and Huxley, so magnificently carrying on the tradition that Cook and Banks had laid down, that the idea of evolution was finally born. In 1958 I had the privilege of delivering a lecture to the Royal Society of Tasmania. The lecture was delivered almost to the day on the centenary of the presentation of Darwin’s and Wallace’s paper on Natural Selection to the Linnean Society of London on 1 July 1858. I reached the somewhat surprising conclusion that the modern idea of ‘evolution was begotten in the union of the Southern Hemisphere with the British Admiralty’. Historically, I believe this to be a reasonable summary. Banks and Cook, with their indefatigable collecting of new geographical facts, and botanical specimens, blazed the trail which so unexpectedly extended man’s intellectual horizons.

Lord Rutherford, another great product of a union consummated in this Southern Hemisphere of ours, is reported to have said, with his characteristic energy and brusqueness, that science could be divided into physics and stamp-collecting. This may have been so. However, any science, including physics, relies on facts observed and patiently collected by accurate but almost always biased investigators, who often know not what they do, particularly in the times before a real body of theory has grown up.

Banks was not a great scientist in the sense of adding substantially to its theoretical structure. He was, like Sir Hans Sloane, whose collections, with Banks’s, formed the early capital of the British Museum, one of the greatest ‘stamp-collectors’ of all time. Linnaeus had laid down in 1735 and 1753, in his Genera Plantarum and Species Plantarum, a theoretical and practical framework which Banks could use in his work of botanical collecting. Banks scarcely added anything to the theory. But throughout his long life he used his considerable personal wealth to add to his collections. He and his wife and sister collected all sorts of things, from old china to caricatures and broadsides, visiting cards, and invitation
cards. In South Kensington is still preserved a book in which Sir Joseph collected the weights of himself and friends. He himself reached a maximum of 17 stone 2 lb!

However, his botanical collections remained his principal interest. He secured the appointment of many of the naturalists to the voyages of discovery undertaken by the British Admiralty. He sent naturalists at his own expense out to the youthful city of Sydney. A good example is George Caley, who collected for Banks in New South Wales from 1799 to 1810—through the turbulent times of Macarthur’s ‘Rum Rebellion’, which deposed another of Banks’s protégés, Governor Bligh. Caley was an undisciplined but also an unfortunate man. In pursuit of his employer’s specimens, he almost succeeded in crossing the Blue Mountains nine years ahead of Lawson, Blaxland, and Wentworth’s successful journey in 1813. There is a delightful tale of Caley’s combativeness, which I quote from Cameron’s biography, Sir Joseph Banks.

Apparently the supply of milk to his botanical establishment at Parramatta had suddenly failed and Caley vented his displeasure on His Excellency, concluding ‘If this sort of treatment is continued I must inform your Excellency that I am an Englishman no longer.’ To this, Governor King has added a note in his own hand ‘The milk trouble is arranged and Caley is an Englishman again.’

Sir Joseph had to use tact and persuasion to keep such men working for him.

Caley, like others of Banks’s collectors, sent back long suites of dried specimens, as well as living plants which Banks grew at King George’s Royal Botanic Gardens at Kew.

The collections of dried specimens were housed in Banks’s house in Soho Square. The house was only pulled down just before World War II to make way for Twentieth Century Fox Films. At his home Banks employed a series of naturalists and artists to work on the job of describing and classifying the specimens. Naturalists included Solander (Linnaeus’s pupil), Dryander, and Robert Brown. Among the artists were Ehret, whose drawings were never published, and Francis Bauer. At his death he left his collections in the charge of Robert Brown, and they subsequently became the basis of the collections of the British Museum (Natural History). In a sense, Kew Gardens and the British Museum (Natural History) represent Banks’s great contributions to the development of science.

Banks’s own scientific work was anecdotal, dilettante, and incomplete. In fact, he reminds me sometimes of a Ph.D. student who has done good experimental work, but who is defeated by the job of writing it up! The British Museum has twenty manuscript volumes by Banks and Solander...
on the botanical results of the *Endeavour*'s voyage, and cards by Banks himself of a projected flora of Iceland, where Banks and his party of scientists collected in 1772, when the accommodation in the *Resolution* proved unsatisfactory to his needs.

Banks kept open house at Soho Square, allowing any qualified person a full run of his great collections. His scientific tea parties and dinners were famous, and he never allowed rank to take precedence over scientific competence.

He was openly and scurrilously lampooned for his pains; but this was a usual thing in those days of Hogarth, Gillray, and Rowlandson. Gillray's cartoon of Sir Joseph after his elevation to the Order of the Bath shows 'The Great South Sea Caterpillar, transformed into a Bath Butterfly'. The cartoon has the following explanatory note added:

This insect first crawled into notice from among the weeds and mud of the Southern Seas and being afterwards placed in a warm situation by the Royal Society was changed by the heat of the sun into its present form. It is noticed and valued solely on account of the powerful red which encircles its body and the shining spot on its breast, a distinction which never fails to render caterpillars valuable.

In 1788 he was savagely lampooned by Peter Pindar on the occasion of 'the approaching Election of a President of the Royal Society', Sir Joseph having been first elected in 1778. In the poet's words, Sir Joseph is confident of re-election, and says:

Yes! Yes! my friend, my tea and buttered rolls
Have found an easy pass to people's souls;
My well timed dinners (certain folks revere)
Have left this easy bosom nought to fear.
Oh tell me fairly, without more delay
What 'tis the blackguard world hath dared to say.

to which the poet's answer is:

This then—'how dares this man his carcase squat
Bold in the Sacred Chair where Newton sat?
When to the Chair Banks forced his bold ascent
He crawled a bug upon his monument.'

To regain balance in this summary, perhaps I can conclude by quoting from Sir Everard Home's Oration, delivered in 1822, soon after Sir Joseph's death. It summarises as well as can be his services to science.

The income of his fortune was expended in the promotion of science and the encouragement of scientific men; and as that income was from time to time increased, he enlarged the scale of his expenditure. He opened his Library, the best which had been formed in books of Natural
Fig. 14. Gillray's cartoon, 'The Great South Sea Caterpillar, transformed into a Bath Butterfly' (by courtesy of the National Portrait Gallery, London)
History to everyone engaged in scientific pursuits desirous of consulting it; he had meetings at his house, but it was not the titled and the wealthy whom it was his object to bring together; it was to form a society of men pursuing science and to bring forward the rising generation, by encouraging in them the same pursuits, and showing to them examples by which they ought to be guided.

Sir Joseph Banks preserved a life-long interest in the botany of the south Pacific. Hence my title. My own interest dates from my appointment as Foundation Professor of Botany in the University of Tasmania. Perhaps a few anecdotes will show how my interest was aroused.

About fifteen years ago, a New Zealand expert on the alpine \textit{Ranunculi} visited us in Hobart. We went out to Strahan on Tasmania’s west coast, not far from where Tasman, in 1642, first glimpsed the land he named Van Diemen’s Land. There are nearby two mountains, Mount Heemskirk and Mount Zeehan, their names commemorating the visit of Tasman’s two ships.

I picked up an obscure little buttercup in the sandhills on that glorious beach facing west towards South America, 10,000 miles away. The New Zealander looked at the plant. It was a new find in Australia—\textit{Ranunculus acaulis} Banks et Solander ex DC. It was first collected by Banks and Solander in November 1769 at the mouth of the Oyster River (Purangi) at Mercury Bay, North Island, New Zealand.

There are some fascinating glimpses of adventures aboard the \textit{Endeavour} in both Cook’s and Banks’s Journals at about the time Solander or Banks collected \textit{Ranunculus acaulis}. Banks says on 8 November: ‘We went ashore and botanized with our usual good success which could not be doubted in a countrey so totally new’. The next day Cook and Mr Green, the astronomer, went ashore early to observe the transit of Mercury. The seamen rather botched their observations. Mr Green, from his lofty eminence as an astronomer, remarks satirically in his log: ‘Unfortunately for the seamen, their look-out was on the wrong side of the sun’.

The Maoris brought to the ship an enormous amount of fish; but later that day the natives turned dishonest. As Cook says, ‘Upon this, Mr. Gore fir’d a Musquet at them, and, from what I can learn, kill’d the Man who took the Cloth; after this they soon went away’. Cook goes on to say that the incident ‘did not meet with my approbation, because I thought the Punishment a little too severe for the Crime’. Cook preferred to blood the Maoris with small shot, which rarely seriously hurt them.

So the white man’s first glimpse of \textit{Ranunculus acaulis} was an exciting one. The plant also occurs in southern Chile, but was not collected there by Banks. So far as I know, it has not been collected anywhere else in the world, including the islands of the southern Indian and Atlantic Oceans. How did the plant become distributed in such a peculiar manner?
Ranunculus is a genus widely distributed in the temperate or alpine regions of both the Northern and Southern Hemispheres. There are a few aquatic species common to both hemispheres. There are a number of genera with a similar bipolar (or amphitropical) discontinuous distribution, for example Euphrasia, the eyebrights, Erodium, the crane's bills, Myosotis, the forget-me-nots, and Viola. Many of them have no species common to both hemispheres.

Coming nearer to home, Allan's Flora of New Zealand (1961) and Curtis's Student's Flora of Tasmania (1956) give forty-three and eighteen species of Ranunculus as occurring naturally in the respective islands. Of these, about seven species, including acaulis, are common to New Zealand and Tasmania. Most of the New Zealand species are endemic to those islands, whilst about six are restricted to Tasmania, although, owing to the recent delimitations of some Tasmanian species, this number may be reduced by discovery of some of the new forms on the Snowy Mountains of southeast Australia.

Ranunculus, in broad outline, illustrates many of the problems confronting the plant geographer. How can we explain the oddities of the distribution of the species? The proper discussion of this problem has, I think, been bedevilled by prejudice and faction. Many authorities almost refuse to look at the possibilities of long-range dispersal, preferring to adopt an hypothesis of age-old land connections in an attempt to explain present-day discontinuities.

However, as P. J. Darlington points out in his closely argued book, Biogeography of the Southern End of the World:

the amphitropical pattern cannot be primarily a product of ancient geography. No biogeographers would seriously suggest (or would they?) that existing north and south temperate areas once formed a single land mass entirely separated from existing tropical areas, and that the amphitropical groups of plants and animals are still distributed according to the ancient division of the land.¹

In other words, bipolar distributions are usually the result of long-range jumps.

Plant geographers have been reluctant to make this deduction, preferring to emphasise the necessity for a continuity of land. For example, there is a long-standing 'principle', to use N. T. Burbidge's words, 'that plant migration has been by advance and retreat of communities and not, at least in the great majority of cases, by chance arrivals'.² Perhaps, because of some innate perversity in my mental make-up, I have over the years collected odd facts in the distribution of plants which make this 'principle' unacceptable to me. The bipolar distribution of Euphrasia, Ranunculus etc., as Darlington says, is one set of contrary
facts. Another is surely the natural distributions which encircle part of the Pacific Ocean. I doubt whether any biogeographer has had the temerity to bring Tasmania and California or Tasmania and Kamchatka or Taiwan into so close a proximity as to allow an advance of a plant community as a whole. Yet there are plant genera distributed discontinuously into these parts of the world. Here are a few examples.

(1) *Boisduvalia* (Onagraceae) is represented in Tasmania by one montane species. There is a closely related one in South America and another group of species in California.

(2) *Oreomyrrhis* (Umbelliferae) crosses the equator on both sides of the Pacific from Taiwan, to Mount Kinabalu in Borneo, to New Guinea, and then to southeast Australia and Tasmania. It is in New Zealand, Fuegia, and the Falkland Islands, and then goes north along the Andes to Mexico and Guatemala.

(3) *Acaena* (Rosaceae), the 'sheep's burr' or 'buzzy' with its barbed fruits, is more widely distributed in south temperate regions, but extends to Mexico and California and Hawaii.

(4) *Lythrum salicaria* (Lythraceae) has an extraordinary distribution, which seems usually to be ignored by plant geographers. It was collected by Robert Brown within a few days of Colonel Paterson's arrival at Port Dalrymple to settle northern Tasmania. So it is a definite native. The species also occurs in eastern Australia almost up to the Queensland border. It jumps the tropics to Kamchatka and Japan, and is also found in Europe and western Asia. The western and eastern populations of Eurasia are not continuous. The species also occurs in eastern North America, but is stated to be introduced there. Cytologically there is another curious complexity. The north Atlantic populations are tetraploid, with sixty chromosomes; the Japanese and Australian are diploid, with thirty chromosomes. The mode of inheritance of the tristyly agrees with the 'ploidy'.

(5) *Gunnera* is a fourth genus which combines a general south Pacific distribution, occurring in Tasmania and the southeast Asian archipelago north to the Philippines, and in South America north to Panama and in the Hawaiian Islands.

The easiest explanation of these odd distributions is that the plants must have jumped from one continent to another.

Another bit of evidence in this connection is worth citing. Several years ago I amused myself listing the genera common to Tasmania and Hawaii. There are sixteen of them, representing about 3 per cent of the genera found on the two islands. They are a curious collection, including
Fig. 15. Migratory route of the Tasmanian mutton bird (*Puffinus tenuirostris*). The two stars indicate the record flight from South Australia to the Bering Sea in forty days (after Serventy).
sedges like *Gahnia* and *Oreobolus*; lilies like *Astelia* and *Dianella*; epacrids (*Cyathodes*); *Pittosporum*, which by some quirk of fate has one species native to the Canary Islands; and so on.

Now, the geologists tell us that the Hawaiian Islands arose, like Aphrodite, from the sea, and have never been in contact with a continental land mass. The genera common to Tasmania and Hawaii are taxonomically a most diverse collection. With one possible exception, *Lagenifera*, they have, however, one characteristic in common. They possess hard stony seeds or fruits. Is this significant? I believe it is.

What vehicles are available to carry the plants across thousands of miles of ocean and land climatically unsuitable to them? I believe the bird-banding programs give part of the answer. Fig. 15 shows the migratory route of the Tasmanian mutton bird (*Puffinus tenuirostris*). As regards speed of migration, I believe the record is still that of a fledgling banded at Goat Island off Ceduna, South Australia, and captured in the Bering Sea, 9,000 miles away, about six weeks after it left the island. The mutton bird is only one of a number of migratory birds travelling north-south across the equator. Others are the common tern (*Sterna hirundo*) and the turnstone (*Arenaria interpres*), which, unlike the mutton bird, breed in the northern hemisphere and fly south to avoid the northern winter.

Other species circumnavigate the world with the Roaring Forties. Fig. 16 gives a picture of some of the recaptures of the giant petrel (*Macronectes giganteus*) banded at Heard and Macquarie Islands. One speed record is from Signy Island in the South Orkneys to Fremantle, about 10,000 miles, in six weeks.

I have neither the time nor that competence in detail necessary to carry this story further. It is high time a good review was produced showing the migratory routes of birds to and from Australia. The data are still scattered. May I also make a plea that the gizzard and stomach contents of any migratory bird be placed in conditions where the germination of any seeds therein will be facilitated? Also, a search should be made or the bird washed externally to detect small seeds like those of *Lythrum* attached externally. Too little of this type of work has been done to evaluate the significance of migratory birds as plant-carriers.

However, there is another possible vehicle, the existence of which was brought to my notice quite accidentally. If you will bear with another anecdote, here are the circumstances.

In 1955 a fisherman in Hobart wished to build himself a new boat. The best timber available in Tasmania, and maybe in the world, is Huon pine (*Dacrydium franklinii*), both names redolent of history. Huon pine has almost been cut out of accessible areas; but, since it is virtually rotproof, it is still possible to find logs, cut fifty or even one hundred years
ago, in the estuaries of west-coast rivers in Tasmania. So the fisherman sailed to Port Davey, selected three good logs, and towed them back to his sawmill in Hobart, where he sawed them up. Two were olfactorily and otherwise Huon pine. The other, a log definitely harvested by man, was not Huon pine, and the sawmiller could not recognise it. It was a nice, clean, light-coloured timber. To cut a long story short, he brought a sample in to the Forestry Commission, who thought this was a good problem for the Professor of Botany. They could not recognise it and, of course, neither could I. So, as Sir Frederick White will agree, we did the obvious thing and sent a sample over to the Division of Forest Products, CSIRO. Dr Dadswell and Mr Ingle identified the log as almost certainly *Nothofagus pumilio* or *obliqua*, probably the former. Both species have characteristic spiral thickenings of the vessels. They form pure stands in southern Chile and Tierra del Fuego, where they are commercially harvested. Had the log escaped from the harvesters and drifted, with the Roaring Forties, 10,000 miles to Tasmania?

I then wrote round the world to the Australian National Antarctic
Research Expeditions in Melbourne, to the Falkland Island Dependencies Survey in London, to Marion Island in the Prince Edward Group, etc., requesting help in collecting driftwood from Macquarie Island, South Georgia, Tristan da Cunha, etc. Fig. 17 gives a partial summary of what we found.4

*Nothofagus pumilio* travels through 235° of longitude. Since this species is not planted commercially anywhere in the world, and since many of the pieces were misshapen thin branches, or the bases of trunks with root-stumps attached, there seems little doubt that we have here another vehicle capable of carrying itself round the world at high southern latitudes. It will take its time. The easterly drift from Cape Horn goes at about eight miles a day, as is proved by numerous bottle drifts.5 A log, to travel from Tierra del Fuego to Macquarie Island, that is about 10,000 miles, would take just over three years. It might go faster if it projected from the water and was ‘sailed’ along by the wind.

The other interesting feature of the driftwood is that it contains specimens of six genera or families (*Drimys? winteri*, *Weinmannia? trichosperma*, an Escallonianaceous plant, *Eucryphia? cordifolia*, *Fitzroya patagonica*, related to the Tasmanian *Diselma*, and an Epacrid) with a
discontinuous present-day distribution into two or more of the southern tips of the continents.

The first three have only drifted from Tierra del Fuego to South Georgia, and are not shown on the map. *Eucryphia cordifolia*, related to the Tasmanian leatherwood, and *Fitzyoa patagonica* are endemic to southern Chile, and have drifted to Macquarie Island. The Epacrid is possibly Tasmanian, in which case it has drifted from Tasmania to South Georgia.

To sum up, all seven types of driftwood belong to groups of plants from the present-day distribution patterns of which some authorities on biogeography would conclude that the continents had drifted, and not the plants.

But I am running ahead of my argument. I am, of course, not suggesting that the logs would sprout up again as cuttings! However, is it quite impossible that a seed might become overgrown in a crevice in the wood or bark, or become trapped in a mass of soil, itself trapped by the growing together or fusing of two roots? We have not been fortunate enough to obtain a living seed from our driftwood.

Perhaps I might conclude by quoting from a letter from Mr S. E. Bishop, who lived in Maui, in the Hawaiian Islands, in 1862. After describing a large tree up to 150 ft in length with roots and branches rising ten feet out of the water, the letter reads:

> I boarded the strange craft, with plenty of standing and living room if needed. Several tons of clayey soil were embedded among its roots, so that I had the pleasure of setting foot once more on American soil, and indulging in the appropriate emotions of patriotism.

This quotation was drawn to my attention by Miss M. Titcomb, Librarian of the Bernice P. Bishop Museum, Honolulu.

Of course, Mr Bishop has let his patriotic fervour get the better of his true facts. What grounds had he for his inference that he was setting foot again on American soil? The sceptic will say none at all. However, Heyerdahl has reported the preference of the Hawaiians in the past for drift logs of Douglas fir (*Pseudotsuga menziesii*) for the construction of their ancient canoes. More recently, C. C. Strong and R. G. Skolmen, two Hawaiian foresters, have published a list of tree species found as driftwood on the beaches of Hawaii. They include 147 specimens derived from western North America and 14 from Southeast Asia or the southwest Pacific. So once again log vehicles are travelling continuously across the oceans.

Lastly, can the seeds themselves drift across the oceans? It is unnecessary here to attempt to summarise Ridley’s classic book or Guppy’s work. Whilst we were chasing driftwood, a few reports of seeds cast up
out of the sea came to our notice. Thus, Mr A. J. Fraser, Director of Fisheries in Perth, and Mr R. D. Royce, Curator of the West Australian Herbarium, reported to us that seeds of *Entada scandens* are often cast up on beaches in southwest Western Australia. In the summer of 1962-3, Royce lists seeds of *Nipa fruticans* (Palmae), *Heritiera* (Sterculiaceae), *Carapa* (Meliaceae), *Sapium* (Euphorbiaceae), and others still unidentified as being cast up on these beaches. All species identified have a wide distribution in the southern tropics. Some unusual set of ocean currents must have brought them several hundred or even thousand miles to the coasts south of Perth.

Similar reports are available for New Zealand and South Africa, where Muir has published an extensive account of seed-drift round the coasts of South Africa.

Many of the species collected in these seed-drifts are leguminous, with thick hard testas, for example *Entada*, *Mucuna*, and *Erythrina*. How many can germinate is still unknown. However, *Entada* seeds carried to New Zealand and Western Australia are still capable of germination.

The distribution of *Edwardsia*, or, as the taxonomists now describe it, *Sophora* series *Tetrapiterae*, may illustrate some of the peculiarities of seed-drift or other methods of long-range dispersal. This is another genus (or subgenus) of the Leguminosae. Again, it has features which some botanists would regard as indicating drifting continents rather than drifting seeds. The thirteen species of the series are distributed as follows: two in Hawaii, one on Lord Howe Island, one on the Chatham Islands, three in New Zealand, one on Masafuera and one on Masatierra in Juan Fernandez Islands, one on Easter Island, one on Reunion in the Indian Ocean, one in temperate South America, and, lastly, a species in temperate South America and Gough Island, near Tristan da Cunha. Thus, only one species is found in two widely separated areas. The other twelve species show a narrow endemism, being restricted to one island or a small part of a continental area.

I have mentioned the details of the distribution of *Edwardsia* because it illustrates a feature common to many of these problems of the botany of the south Pacific—the fact that few if any species of genera widely distributed into the southern tips of the continents are common to more than one area. *Nothofagus*, as we shall see later in more detail, and *Astelia* are classical examples. It has given rise to what I may refer to as ‘Skottsberg’s problem’. Skottsberg, who died in 1963 at the age of eighty-three, had as wide an experience botanising in the Southern Hemisphere as anyone. His first trip was in 1901, when he accompanied Nordenskjold as botanist on the Swedish Antarctic Expedition. He became convinced, like Joseph Dalton Hooker before him, that it was necessary to postulate a temperate Antarctica to serve as a centre for the origin of his ‘Austral’
or ‘Palaeoantarctic’ flora, which was then distributed into the southern ends of the continents, as Antarctica froze and became inhospitable to higher plants. In a characteristic passage in his paper to the Royal Society in 1959 on the occasion of ‘A Discussion on the Biology of the Southern Cold Temperate Zone’, he says:

The Swedish Antarctic Expedition discovered fossil-bearing sediments with numerous leaf impressions belonging to two different horizons, one Jurassic, the other presumably Oligocene and containing genera now living in South America and New Zealand. When, in 1906, I met Sir Joseph [Hooker], I told him of what we had found. He was pleased.

Skottsberg’s problem was to account for the endemism, if the dispersal had been across long stretches of ocean in a bird’s gut or embedded in a log. In one paper he discusses the problem as though the mechanisms for long-range dispersal may have been more efficient in early Tertiary. I doubt whether there is any evidence for this. Surely the explanation may lie in the known or postulated laws of natural selection. A small inoculum, to import a term from mycology, of a species unrelated to any already existing in a community of plants, could be in a genetically very different situation from the same inoculum attempting to invade a community already well-stocked with related species. There is increasing evidence that an important deduction first made by R. A. Fisher in his classical _Genetical Theory of Natural Selection_ is usually true. Fisher’s deduction was that genetic sterility barriers are created between related species for a simple adaptive reason. The adaptive or selective reason is that a genetic sterility barrier between two related species prevents the rapid breakdown by hybridisation of two adapted genotypes, already tested by selection. The new combinations produced by hybridisation will be untested. The great majority will be less fitted to survive than the two parental and tested gene combinations. Clearly, the development of a genetic sterility barrier can thus only be expected where the chance of interspecific hybridisation is high, as for two sympatric species. Where the chance is low, and brought about by the occasional arrival of a seed from long distances by any of the vehicles we have been discussing, we cannot expect the origin by natural selection of a genetic sterility barrier. The arrival of the new immigrant will be almost equivalent to a multiple mutation occurring in the population. It will scarcely do more to alter the course of selective evolution than will the mutations spontaneously occurring in the already established population.

I have previously discussed some of the evolutionary systems where Fisher’s deduction may be invalid. They include speciation by polyploidy when a genetic sterility barrier arises coincidentally with the speciation; and systems of selection, where the cost of that selection is
almost wholly debited against a density-dependent death-rate, obtaining particularly during the seedling or larval phases of the population. Neither of these limitations need worry us in the present context, although for this and other reasons it is pleasing that Moore and others are starting to investigate the cytology and genetic systems operating in genera showing both an extraordinary discontinuity in distribution and a high degree of endemism.

It is obvious that successful long-range dispersal will place the inoculum in a new environment, both climatically and biotically. Thus, natural selection may be expected usually to cause a rapid change in genotype and phenotype, if genetic variation in the new population is sufficient for selection to act on. Genetic variation in the new population will, at first, be limited to that brought in by the inoculum. This may be considerable in a previously outcrossing population. It will immediately be expressed phenotypically because of the inbreeding consequent on the initially small population numbers. Whether the operation of this effect is to be seen in the origin of so many flightless forms of insect located on small oceanic islands, is still not clear. However, with the growth in population, particularly in forms producing large numbers of seeds or eggs, mutation will in a few generations produce sufficient variation for the continuing evolution of the new endemic form.

Thus, unless we are prepared to postulate very large and inexplicable variations in the rates of evolution by natural selection, the presence of the same species in widely separated parts of the world is prima facie evidence of a fairly recent immigration from one centre to another. Ranunculus acaulis has probably reached its present range of Tasmania, New Zealand, and Tierra del Fuego in the last million or so years by one or other of the vehicles discussed above.

In this conclusion, I am, perhaps, only agreeing with that reached by a man who was a boy of eleven when Sir Joseph Banks died in 1820. I refer to Charles Darwin. The following passage, from Chapter XII of On the Origin of Species, is a long one, but difficult to summarise. It illustrates so well Darwin’s mastery of the sort of detail which would have fascinated Sir Joseph Banks that I quote the whole passage.

Seeds may be occasionally transported in another manner. Drift timber is thrown up on most islands, even on those in the midst of the widest oceans; and the natives of the coral islands in the Pacific procure stones for their tools, solely from the roots of drifted trees, these stones being a valuable royal tax. I find that when irregularly shaped stones are embedded in the roots of trees, small parcels of earth are frequently enclosed in their interstices and behind them,—so perfectly that not a particle could be washed away during the longest transport: out of one small portion of earth thus completely enclosed by the roots of an oak
about 50 years old, three dicotyledonous plants germinated: I am certain of the accuracy of this observation. Again, I can show that the carcases of birds, when floating on the sea, sometimes escape being immediately devoured: and many kinds of seeds in the crops of floating birds long retain their vitality: peas and vetches, for instance, are killed by even a few days' immersion in sea-water; but some taken out of the crop of a pigeon, which had floated on artificial sea-water for 30 days, to my surprise nearly all germinated.

Living birds can hardly fail to be highly effective agents in the transportation of seeds. I could give many facts showing how frequently birds of many kinds are blown by gales to vast distances across the ocean. We may safely assume that under such circumstances their rate of flight would often be 35 miles an hour; and some authors have given a far higher estimate. I have never seen an instance of nutritious seeds passing through the intestines of a bird; but hard seeds of fruit pass uninjured through even the digestive organs of a turkey. In the course of two months, I picked up in my garden 12 kinds of seeds, out of the excrement of small birds, and these seemed perfect, and some of them, which were tried, germinated. But the following fact is more important: the crops of birds do not secrete gastric juice, and do not, as I know by trial, injure in the least the germination of seeds; now, after a bird has found and devoured a large supply of food, it is positively asserted that all the grains do not pass into the gizzard for twelve or even eighteen hours. A bird in this interval might easily be blown to the distance of 500 miles, and hawks are known to look out for tired birds, and the contents of their torn crops might thus readily get scattered. Some hawks and owls bolt their prey whole, and, after an interval of from twelve to twenty hours, disgorge pellets, which, as I know from experiments made in the Zoological Gardens include seeds capable of germination. Some seeds of the oat, wheat, millet, canary, hemp, clover, and beet germinated after having been from twelve to twenty-one hours in the stomachs of different birds of prey; and two seeds of beet grew after having been thus retained for two days and fourteen hours. Fresh-water fish, I find, eat seeds of many land and water plants; fish are frequently devoured by birds, and thus the seeds might be transported from place to place. I forced many kinds of seeds into the stomachs of dead fish, and then gave their bodies to fishing-eagles, storks, and pelicans; these birds, after an interval of many hours, either rejected the seeds in pellets or passed them in their excrement; and several of these seeds retained the power of germination. Certain seeds, however, were always killed by this process.

How many discontinuities of plant distribution in the south Pacific are to be explained in this way by long-range jumps across the oceans I do not know. The vehicles exist and have existed for over seventy million years, that is throughout Tertiary times, and well into the Mesozoic.
However, let us look at the situation in the second group of plants I mentioned earlier—the genus *Nothofagus*, the southern beeches.

In January 1769 Cook took the *Endeavour* through the Strait Le Maire, between Staten Island and Cape San Diego on Tierra del Fuego. He tried to anchor a few miles west of the Strait on 15 January, but decided there was too much risk. However, he sent a boat ashore,

to attend to Mr. Banks and people who was very desirous of being on shore at any rate, while I kept plying as near the shore as possible with the Ship. At 9 they return'd on board bringing with them several Plants, Flowers, etc., most of them unknown in Europe, and in that Alone consisted their whole Value.

Thus speaks the practical navigator! However, Cook is, perhaps, a little too sweeping. Banks in his Journal for this day has the following passage:

Among other things the bay affords there is plenty of winters bark, easy to be known by its broad leaf like a laurel of a light green colour and blueish underneath, the bark is easily stripd off with a bone or stick as ours are barkd in England; its virtues are so well known that I shall say little except that it may be us'd as a spice even in culinary matters and is found to be very wholesome. Here is also plenty of wild celery *apium antescorbuticum*, scurvy grass *cardamine antescorbutica*, both of which are as pleasant to the taste as any herbs of the kind found in Europe and I believe possess as much virtue in curing the scurvy.

The trees here are chiefly of one sort, a Kind of Birch *Betula antarctica* with very small leaves, it is a light white wood and cleaves very straight; sometimes the trees are 2 or 3 feet in diameter and run 30 or 40 feet in the bole; possibly they might in cases of necessity supply topmasts. Here are also great plenty of cranberries both white and red, *Arbutus rigida*.

Winter's bark is *Drimys winteri*, which we have already met in the driftwood of South Georgia. Its bark is antiscorbutic. Curiously, *Drimys winteri* was so named by Forster, naturalist on Cook’s second voyage, after Captain John Winter, who sailed with Drake in the *Golden Hind*, and who successfully used the plant to combat scurvy when the ship was in the Straits of Magellan in 1578. The wild celery is now *Apium prostratum*, which, as Banks says later, ‘resembles much the Celery of our gardens . . . and is not easily mistaken as the taste resembles Celery or parsley or rather is between’. *Cardamine antescorbutica* is one of the ‘scurvy grasses’ of the old mariners. It is now *C. glacialis*. Both genera are amphitropical in distribution and also occur naturally in Australasia. The berries of *Arbutus rigida* (now *Pernettya mucronata*) are, as Skottsberg found during his travels in the wilds of Patagonia, useful as emergency rations. Thus Cook is, perhaps, less than just in his summing up of the usefulness of Mr Banks’s first collection of plants from the southern tip of South America.
I have left Banks's *Betula antarctica* to the last. This is now called *Nothofagus betuloides*. I use here Dr W. T. Stearn's statement in a letter to me. Beaglehole, in his edition of *The Endeavour Journal of Joseph Banks*, equates it to *N. antarctica*. Banks did not find flowering material.

Cook finally found a good anchorage a few miles further west in the Bay of Good Success. Banks and a party, including Solander and Green, immediately set off to

try to penetrate into the countrey as far as we could, and if possible gain the tops of the hills where alone we saw places not overgrown with trees.

However, when the party reached these places not overgrown with trees, they found they were

no better than low bushes of birch about reaching a man's middle; these were so stubborn that they could not be bent out of the way, but at every step the leg must be lifted over them and on being plac'd again on the ground was almost sure to sink above the anckles in bog. No travelling could possibly be worse than this. . . .

I might add, except, possibly, a thicket of *Nothofagus gunnii* on the Tasmanian hills. *N. gunnii* has received the expressive vernacular name of 'tanglefoot'. It does just this very effectively.

It was on this trip that Banks and party were overtaken by a snow-storm, during which Banks's two negro servants lagged behind and, after consuming a bottle of rum, perished with cold. Banks made strenuous efforts to save them and others. He successfully rescued the artist, Buchan, who had suffered an epileptic fit earlier in the day, and Dr Solander, who at one point collapsed with extreme fatigue.

Darwin was the next naturalist to collect round the Bay of Good Success, when the *Beagle* visited it in December 1832. He complains, like Banks, of the appalling climate in summer. Joseph Dalton Hooker, who also collected nearby in 1842, is not nearly so sympathetic to Banks's difficulties. But Hooker had come from the Ross Sea and Tasmania, not from Rio de Janeiro. I can testify that it is more than disconcerting to camp out in the Tasmanian highlands just before Christmas and wake to a silent, snow-covered world.

At the Bay of Good Success Banks's party collected a second *Nothofagus* species—*N. antarctica*—which was in flower, so they could refer it to the then correct Linnean genus, *Fagus*.

On 26 January Cook was bound from Tierra del Fuego to Point Venus, on Tahiti. In the ten days available to them Banks and Solander had made their first and notable contribution to the botany of the Southern Hemisphere.
As we have seen, twelve months later the *Endeavour* was on the coasts of New Zealand. Banks collected two species of *Nothofagus*; but they consisted of vegetative material only. Solander gave the two suites of specimens the token names, *Cliffortioides oblongata* and *Myrtilloides cinerascens*. There is no indication that he and Banks recognised their botanical relationship to the South American plants. This is not surprising, because I was told by Mr John Womersley, forest botanist at Lae, New Guinea, when he took me to see *Nothofagus* growing at Edie Creek, out from Bulolo, that when the first specimens were procured in the 1920s the tri-locular ovary suggested a relationship to the Euphorbiaceae. So for incomplete specimens a very wide margin for error exists even among the best of taxonomists!

Banks and Solander collected their specimens of the two species of *Nothofagus* in Queen Charlotte’s Sound at the northern end of the South Island. It was here the *Endeavour* came across plain evidence that the Maoris were anthropophagi or cannibals. Banks says:

Tho we had from the first of our arrival upon the coast constantly heard the Indians acknowledge the custom of eating their enemies we had never before had a proof of it, but this amounted almost to demonstration: the bones were clearly human, upon them were evident marks of their having been dressd on the fire, the meat was not intirely pickd off from them and on the grisly ends which were gnawd were evident marks of teeth, and these were accidentaly found in a provision basket. On asking the people what bones are these? they answerd, The bones of a man.—And have you eat the flesh?—Yes.—Have you none of it left?—No.—Why did not you eat the woman we saw today in the water?—She was our relation.—Who then is it that you do eat?—Those who are killd in war.—And who was the man whose bones these are?—5 days ago a boat of our enemies came into this bay and of them we killd 7, of whom the owner of these bones was one.

Later he goes on:

We made another excursion today. The bay every where we have yet been is very hilly, we have hardly seen a flat large enough for a potatoe garden. Our freinds here do not seem to feel the want of such places as we have not yet seen the least appearance of cultivation, I suppose they live intirely upon fish dogs and Enemies.

However, Banks's love of nature also shines through his journal.

This morn I was awakd by the singing of the birds ashore from whence we are distant not a quarter of a mile, the numbers of them were certainly very great who seemed to strain their throats with emulation perhaps; their voices were certainly the most melodious wild musick I have ever heard, almost imitating small bells but with the most
tuneable silver sound imaginable to which maybe the distance was no small addition. On enquiring of our people I was told that they had observed them ever since we have been here, and that they begin to sing at about 1 or 2 in the morn and continue till sunrise, after which they are silent all day like our nightingales.

The bird is the New Zealand bellbird (*Anthornis melanura*), as Professor Beaglehole comments in a footnote. He goes on: ‘But alas! that chorus of melodious music is no longer heard where he [Banks] heard it’.

After leaving New Zealand on 1 April 1769, the *Endeavour* cruised up the east coast of Australia, landing at Botany Bay on 28 April, where they stayed until 5 May. On 23 May they anchored in Bustard Bay, a few miles south of what is now the thriving aluminium town of Gladstone. On 26 May they anchored in Keppel Bay, just north of the site of Rockhampton; on 29 May they were at Thirsty Sound, across Broad Sound from the present township of St Lawrence. The *Endeavour* sailed past Pentecost Island, through Whitsunday’s Passage and Trinity Bay, all of which are Cook’s names, telling us his progress up the coast, to the near tragedy of the Endeavour Reef, and to the repair at the Endeavour River. After the repair the party landed, for Cook to look for a passage through the reefs at Point Lookout, which is only about fifty miles north of the Endeavour River. The ship’s last landing place in Australia was at Possession Island, not far from Cape York, on 21 August 1770.

Although Banks and Solander obtained a magnificent series of specimens, they did not and could not have collected any Australian *Nothofagus*. The genus in Australia consists of three species. *N. gunnii* is confined to the mountains of Tasmania; *N. cunninghamii* is native to Tasmania and Victoria; whilst *N. moorei* occurs on the hills from just north of the Hunter Valley to the Lamington Plateau on the Queensland-New South Wales border. The genus thus has a discontinuous distribution in Australia.

A distribution of this type immediately makes one think of a relic distribution. Has the genus previously had a wider distribution? Ecologists, like Herbert, have pointed out that a relatively small change of climate could lead to the retreat of the eucalypts and the expansion of *Nothofagus* or the tropical Indo-Malayan elements in the Australian flora.

Fortunately we have not to rely solely on ecological speculation. Fig. 18 also shows the fossil distribution of *Nothofagus* in Australia. This is based largely on the discovery of fossil pollen grains. The record is incomplete. For example, very few deposits have been examined in Queensland or anywhere in tropical Australia. It is apparent that *Nothofagus* was widespread in Australia until late in the Tertiary, extending from the southwest to the southeast.
I have also indicated another important feature of the distribution of Nothofagus on the map. The genus contains three pollen types, called respectively brassi, fusca, and menziesii, after typical species in which they occur.

As you can see, the present-day Australian species include one fusca type, which is the Tasmanian $N. \text{gunnii}$; and two menziesii types, $N. \text{cunninghami}$ and $N. \text{moorei}$. Brassi type does not occur. Today, in fact, brassi type occurs only on the mountains of New Guinea and of New Caledonia. However, this type of pollen was widespread in southern Australia, probably until middle Tertiary. Similarly, fusca type had a far wider distribution. It persisted longer on mainland Australia than brassi. The latest occurrence has been reported by Miss Helene Martin, working in my department at the University of New South Wales. Unfortunately we cannot date very accurately the water-bearing gravels from which she obtained her pollen grains. They are, however, certainly later Pliocene and less than five million years old.
Fig. 19 shows drawings of the three types of pollen, and pollen from the northern hemisphere *Fagus*, which, as you can see, has a very different type of pollen from any of the *Nothofagus* species.

What is the present-day world distribution of the genus *Nothofagus*? Fig. 18 also indicates what pollen-types occur. Thus, some of the world discontinuity is due to extinction. Fifty or sixty million years ago, parts of Antarctica were forest-covered. There is a good fossil flora at Seymour Island, worked on by the Swedish Antarctic Expedition, containing *Nothofagus* and several other genera with a discontinuous distribution into the southern tips of the continents. More recently *Nothofagus* pollen grains have been discovered at McMurdo Sound.

The prehistoric distribution of *brassi*-type pollen is extremely interesting. It has occurred in all areas from Seymour Island to McMurdo Sound.

Fig. 19. *Nothofagus* and *Fagus* pollen grains (after Cookson, Harris, and Wodehouse)
to New Zealand and Australia. Unfortunately, we have no data for early Tertiary for New Guinea. Did it come down from New Guinea, which Darlington would like to think? Or is it more likely that brassi retreated north with the worsening of the climate during late Tertiary? All we can say with certainty at present is that the extinction of brassi Nothofagus in Australia occurred in Miocene, ten to twenty million years ago. Fusca pollen, as Miss Martin has shown, is still present in the Lachlan River gravels, less than five million years old. It is accompanied, strangely enough, by Dacrydium pollen, not of the Tasmanian D. franklinii type, but of the New Zealand and New Guinea form. R. A. Couper10 has shown a somewhat similar pattern of extinction in New Zealand. However, brassi is still present into the lower Pleistocene and, of course, fusca is still living there.

On balance, it appears to me more likely that Nothofagus is an evolutionary product of our Southern Hemisphere. Is it really closely related phylogenetically to the northern hemisphere Fagus, as Darlington assumes?

The scanty chromosomal data available are set out below. The old family Cupuliferae was divided by the taxonomists into three families, the Betulaceae, the Corylaceae, and the Fagaceae.

**CHROMOSOME NUMBERS IN THE CUPULIFERAE**

- Betulaceae $x = 14$
  - Alnus $x = 14$
    - $2x$, $3x$, and $4x$
    - in 15 sp.
  - Betula $x = 14$
    - $2x$, $3x$, $4x$, $5x$, $6x$
    - in 24 sp.

- Corylaceae $x = 8$
  - Carpinus $x = 8$
    - $2x$ and $8x$
    - in 8 sp.
  - Ostrya $x = 8$
    - $2x$ in 4 sp.
  - Corylus $11? 14?$
    - $2x$ in 10 sp.

- Fagaceae $x = 12$
  - Castanea $x = 12$
    - $2x$ in 4 sp.
  - Fagus $x = 12$
    - $2x$ in 1 sp.
  - Quercus $x = 12$
    - $2x$ and $4x$ in 28 sp.
  - Nothofagus $x = 13$
    - $2x$ in 2 sp. in N.Z.

Thus, in general, the cytology agrees with the taxonomists' judgment, which is satisfactory to all concerned. There are two difficulties. The first is that the counts in Corylus require checking. The second is Nothofagus. If the counts are correct, and Armstrong and Wylie's work11 leaves little doubt about $n = 13$ in *N. solandri* var. cliffortioides, is the genus properly placed? In other words, is it closely related to the rest of the Fagaceae? Clearly we must have more cytological data, and perhaps an up-to-date morphological and anatomical study of the reproductive structures may suggest a convergent evolution.

I mentioned above that Darlington suggests a northern origin for the
Fig. 20. Specimens of *Nothofagus truncata* collected by Banks and Solander, Queen Charlotte's Sound, New Zealand, January 1770
Fig. 21. Specimens of *Nothofagus fusca* collected by Banks and Solander, Queen Charlotte’s Sound, New Zealand, January 1770
CAPTAIN COOK

genus. He hopes that, perhaps, in Siberia Fagus and Nothofagus pollen may be found together in Cretaceous rocks. He argues that the oaks seem to be crossing the equator now. I can remember being vividly confronted with this fact when Mr Womersley told me we would visit an oak forest a few miles out of Lae in northern New Guinea. I immediately asked, ‘What kind of oak?’—bearing in mind our Australian new uses for this old northern name. Australian oaks range from the silky oak (Proteaceae), bull-oke and she-oke (Casuarina), to Tasmanian oak (Eucalyptus regnans, obliqua etc.).

Womersley replied, ‘Real native oaks’, and there they were, with good acorns, but largish evergreen entire leaves, mimicking the tropical rainforest trees. They grow in company with tropical Dipterocarps and southern Podocarps. The evidence all suggests that the oaks have migrated from the north.

However, as Florin has proved, using good palaeobotanical evidence, among the conifers there is a southern group which has been separated from the northern since early Mesozoic or late Palaeozoic. Some have crossed the equator (e.g. Podocarpus). I prefer to build on this analogy and to assume a southern origin for Nothofagus.*

Even if we assume this, we are scarcely ‘out of the wood’ with regard to the difficult problems posed by the geological and geographical facts known about Nothofagus. However, whether it is my liking for history or not, I believe that Sir Joseph Hooker’s postulation of a temperate Antarctica, which, if you like, served as an early Tertiary stepping stone for Nothofagus to migrate to the southern tips of the continents, is as good a hypothesis as any yet available. In view of the isostatic sinking of Antarctica under its present mass of ice we cannot be sure how far the longer range ‘jumps’ would have to be. However, as we have seen, the vehicles do exist and have been there for the hundred million years the genus Nothofagus has lived on this earth of ours. Darlington also discusses the possibility of wind distribution for some of the smaller seeded species which also have winged seeds.

Can continental drift have helped these wanderings of Nothofagus round the southern tips of the continents? K. M. Creer’s12 reconstruction (Fig. 22) shows Antarctica placed between South America and Australia, with South Africa not far away from the narrow strait between Australia and Antarctica.

His map is based on the remanent magnetisation in dolerites from six localities in South Africa, Antarctica, Australia, India, and South

* There have been reports of ‘Nothofagus’ pollen grains in the London clay. Recent evidence suggests that these are probably misidentified pollen grains of the Chloranthaceae.
America. The ages of most of these dolerites have been determined radiometrically since Creer's map was published. They vary from 119-47 (South America) to 154-90 (South Africa) million years. The position of Australia is based on the remanent magnetism of the Tasmanian dolerites, which have an age of 167 million years. If we accept this age, Creer's map gives the situation of the continents about 170 million years ago. The earliest Angiosperm fossil is about 120 million years old and the earliest Nothofagus fossil is 90 million years old. So there is a 'time-gap' of at least 50 to 80 million years. Perhaps later work will reduce this gap and allow us to conclude that continental drift helps to explain the present-day distribution of Nothofagus.

In parenthesis, I should mention that Nothofagus is almost the classical case for those who believe in the principle that 'plant communities
migrate as a whole'. Gordon, in his Presidential Address to the Botany Section of ANZAAS in Hobart in 1949, first introduced me to this idea. It is true that, in Tasmania today, destruction of a *Nothofagus* forest by felling may not be followed by recolonisation of the rain-forest, even though conditions would apparently seem favourable. For example, what I have elsewhere referred to as the Parawee 'desert' in northwestern Tasmania is still a mass of dead *Nothofagus* logs, almost fifty years after felling. Rabbits, bracken, fire, and a slight amount of rough grazing, together with possibly the destruction of the right microclimate for seedling establishment, seem to have prevented the rain-forest community from advancing.

It is also true that associated with *Nothofagus* are usually (but not always) an extraordinary ascomycete fungus, *Cyttaria*, and an even more extraordinary primitive group of bugs, the Peloriidae. This association has been used to buttress the community migration idea. However, as Darlington points out, Peloriids occur on Norfolk Island, which is apparently a strictly volcanic oceanic island, and on Stewart Island, south of New Zealand. *Nothofagus* does not occur on either Norfolk or Stewart Island. The absence of *Nothofagus* from Stewart Island is a curious, inexplicable fact. Foveaux Strait is less than thirty miles wide. *Nothofagus* occurs across the strait on the south island. The strait is less than 100 ft deep and *Nothofagus* occurred on Stewart Island a few thousand years ago.

Thus, this analysis of what data there are points to a similar origin for the distribution of *Nothofagus* as we arrived at for *Ranunculus acaulis*—long-range dispersal followed by a strict climatic and ecological sifting. The only difference between the two cases may be time, *R. acaulis* migrating in the last few million years, whilst *Nothofagus* migrated perhaps a hundred million years ago.

My last example of a discontinuous distribution into the southern tips of the continents was the family Proteaceae. It is just possible that here we have, at least in part, evidence of the break-up of Gondwanaland. The South African group is taxonomically quite separate from the Australian group. No genus occurs in both areas. Some of the few South American species are congeneric with Australian. Are they relics of a temperate Antarctic connection like *Nothofagus*? Are the African and Australian species relics of the break-up of Gondwanaland in the Mesozoic, as perhaps Creer's reconstruction may help us imagine? I do not know; but it is, I think, at this taxonomic level that we should seek botanical evidence, if it exists, that the history of drifting continents still determines the distribution of some plants.

However, I have said enough to allow you to see that more patient 'stamp-collecting' by old-fashioned naturalists using such modern tools as
palynology, cytology, and genetics may help us answer some of these fascinating problems, which Banks and Solander started to define for us. Is it any wonder that I have a small corner of my garden which I call my Banksian garden? In it I have growing *Rhododendron luchae* from Mt Bellenden-Kerr in northern Queensland. It is our only native *Rhododendron* far away from the centre of diversity of the genus in the Himalayas. I also have *Eucryphia moorei* from the south coast of New South Wales, *Eucryphia billardieri* from Tasmania, and *Eucryphia cordifolia* from Chile. And *Araucaria auracana* from Chile grows side by side with *Araucaria cunninghamiana* from northern New South Wales, with two species of *Protea* from South Africa next door to *Macadamia* and *Grevillea robusta* from Queensland and northern New South Wales, and *Knightia* from New Zealand.

I have at last obtained *Nothofagus pumilio* from Tierra del Fuego, which is growing with Tasmanian *N. cunninghami*. I hope to get a New Guinea *Nothofagus* as well as a New Zealand one.

Last, but not least, I hope the beautiful plant, *Aristolelia peduncularis*, will survive. It is a native of Tasmania’s rain-forest. Other species of the genus occur in New Zealand and Chile. So, if by chance you find yourself in Tierra del Fuego, send me some seed.

As my Banksian garden grows to a convenient height for my declining physical powers to cope with, I may try a few tests for the existence of genetic sterility barriers between related species so long separated. I may, perhaps, knock one small nail into the coffin of those of my botanical colleagues who believe the continents must always drift faster than the plants can travel!

I wish to thank Dr W. T. Stearn for his help in determining where Banks’s specimens were collected, and their nomenclature.
THE SIGNIFICANCE OF THE TRANSIT OF VENUS

Sir Richard Woolley

The voyage of the bark *Endeavour*, the bicentenary of which we are now celebrating, was organised by the Royal Society for the purpose of determining accurately the solar parallax by the method of observing the duration of the transit of Venus across the disc of the Sun, from points widely separated in latitude on the Earth’s surface.

Some explanation is needed as to why the scientists of the eighteenth century thought that the correct evaluation of the solar parallax was so important as to demand the commission of so expensive and indeed so hazardous a voyage.

To set out the matter through contemporary eyes I shall quote extensively from *Histoire Abrégée de la Parallaxe du Soleil*, written by Jacques Dominique Cassini and published in 1772 in Paris, as part of the *Voyage en Californie* which he ascribed to the Abbé Chappe, and edited after the Abbé had died.

Cassini is the name of a family of distinguished astronomers who succeeded one another as Directors of the Observatory of Paris for four generations. The first and most famous Cassini was Giovanni Domenico, who was born near Nice in 1625. He became Director in 1671, and was followed as Director by his son, his grandson, and by our author, his great grandson, Jacques Dominique Cassini, whose office was actually terminated by the events of the French Revolution, that is to say in 1793, 122 years after the first Cassini had begun. In 1793 the National Assembly, who of course saw Cassini as an Establishment figure, decreed that three of his pupils should be united with him in his office of Director, an arrangement to which he refused to submit. I hope that I shall show the same resolution if and when my day comes. Cassini was imprisoned for seven months, and after this he abandoned astronomy and lived in retirement.

Of the solar parallax, he writes:

The parallax of the Sun is one of the points of Astronomy which has deeply engaged scientists for about a century. The influence of this
quantity on the whole planetary system makes the inquiry extremely important. One need not therefore be surprised at the numerous investigations and the arduous journeys, which have been undertaken for a number of years, nor by the eagerness which the most enlightened nations of Europe have shown to contribute individually to the success of this interesting research.

Astronomy, like every other science which is founded solely on observation and the collection of facts, can but have slow progress. It must depend on the lapse of time. A happy stroke of genius can hasten its march to perfection by a few steps: but there are some discoveries which await events which nothing at all can accelerate, such as the passing of Venus in front of the Sun's disc. For this alone could dissipate absolutely our uncertainties on the value of the solar parallax; this alone could fix, with the utmost precision, a quantity which had varied up to the present, according to the opinions of various astronomers and to the different methods employed in the determination. Happy is our century for which has been reserved the glory of being the witness of an event which will forever make it memorable in the annals of Science!

But exactly what has been the difficulty, up to the present, of determining the solar parallax? What has made astronomers differ on this point, and made their methods inadequate? How does the transit of Venus give a result better than all other methods, indeed free of all uncertainty? Finally, what conclusions have been drawn? These questions have naturally occurred to all those who have heard talk of the transit of Venus, and who, from a taste for science, have turned their minds to it. I propose to satisfy their curiosity here, and to explain as clearly as I can, all that one can desire to know on the subject; but I do not propose to enter into all the details, which demand a special treatise, and are outside the limits of the article, which can only be considered as an introduction.

The parallax of the Sun is the difference between the place in which the object appears when seen from a point on the Earth's surface, and the place in which it would appear if seen from the centre of the Earth: or, if you wish, it is the angle subtended by the radius of the Earth at the centre of the Sun. This is the angle at the vertex of a triangle which has as a vertex the centre of the Sun and as base the radius of the Earth.

One can appreciate at once that this angle is the smaller, the further the Sun is away from us. The solar parallax is in fact dependent on its distance from the Earth and if this distance was known we could calculate the parallax and conversely. But in the parallactic triangle we only know one side, the radius of the Earth, and there is no means of measuring another side directly. This is the difficulty.

This difficulty defeated the ancient astronomers and remained for a long time too much for their resources; they were reduced to conjectures. Petofiris and Necepsos, Kings of Egypt, believed that the
Earth was distant from the Sun by 2970 stadia (which is only 130 leagues). Pythagoras, I don't know by what reasoning, fixed the distance as eighteen thousand leagues. These opinions, which as one sees are enormously removed from the truth, and which we regard today as ridiculous and absurd, were followed as correct in those early ages when knowledge either of theory or of practical astronomy was too limited to be able to offer anything better. It was not until the year 264 B.C. that anybody had any less crude ideas on this subject, and these were due to Aristarchos of Samos. The improvement which this philosopher brought to the opinion of his predecessors, although far from perfect, is nevertheless worthy of high praise since it is based on a very ingenious method, of which this is the idea.

Aristarchos who supposed that the distance from the Earth to the Moon was known, wanted to measure the elongation between the Sun and the Moon at the moment of quadrature. This would give one side and one angle in a right angle triangle, of which the distance between the Earth and the Sun would be the hypotenuse, and would therefore be easy to calculate. By this means Aristarchos settled it that the solar parallax could not exceed three minutes of arc. This determination indeed is twenty-one times too large, but Aristarchos could hardly attain any greater precision. He supposed the distance between the Earth and the Moon known, and it was very badly determined. Besides, however rigorous his method is in theory it is very difficult to apply in practice, so far as determining the moment of quadrature exactly is concerned, that is the moment when the angle of the Moon is just 90° which can only be judged from the illuminated half only. Besides the aspect of the Moon is subject to small variations . . . Nevertheless by using this method Riccioli and Vendélius have got quite close to the true value of the solar parallax.

The only alternative method known to the ancients was one suggested by Hipparchos and this consists in determining the diameter of the Earth's shadow during an eclipse of the Moon. Though the method is sound geometrically it is almost useless in practice as the Earth's shadow is far from sharp, on account of refraction in the Earth's atmosphere, and the diameter of the Earth's shadow on the Moon cannot be accurately measured. In about 1650, therefore, the scientists would have had to choose between the best determinations made with Aristarchos's method, and these were, by Vendélius 15°, and Riccioli 28°. This was about the time that the Royal Society received its charter, and the Paris Academy was founded. Practical astronomy in particular was studied by such illustrious scientists as Flamsteed and Halley in England, and by the first Cassini and Roemer in France.

The results which had been obtained up to that date sufficed to show that the solar parallax was a very small quantity, and beyond the reach of observations whose own errors could exceed it and obscure it,
thus making it most difficult to determine properly. It therefore appeared much more natural to have recourse to the planets, such as Mars and Venus, whose parallax would be much larger than that of the Sun, and consequently much more easy to determine. Once the parallax of one of these planets was known it would be easy to determine that of the Sun. Indeed, the theory of planetary motion tells us what the relative distances are between the Sun and all the planets, including the Earth; and the parallaxes are proportional to the inverses of these distances.

To get the solar parallax, therefore, one has only to determine with the greatest possible accuracy the parallax of one planet.

The displacement due to parallax, which depends on the difference between the planet's geocentric position and its position when seen by an observer on the Earth's surface, is zero when the observer is directly between the centre of the Earth and the planet, that is to say when the planet is in the observer's zenith; and the parallactic displacement is always towards the zenith. There are two practical ways of determining the parallax of a planet such as Mars:

(1) When the planet is in the meridian, the displacement is the declination only. Two observers on the same meridian but in different latitudes, one in the Northern Hemisphere and one in the Southern, will observe different apparent declinations of the planet. The difference will give the parallax.

(2) At any one station there will be apparent changes in the right ascension of the planet dependent on its distance from the meridian, and these will give the parallax.

Some observations of the declination of Mars were made in Cayenne in 1672 by Richter. These were compared by J. D. Cassini with observations made in Paris, and he deduced a parallax of 25° for Mars, and therefore about 10° for the solar parallax; Flamsteed agreed with Cassini, but other astronomers produced quite different results. Monsieur Halley, a little too prejudiced against all methods of determining the solar parallax except that using the transit of Venus, was one of those who did not admit Flamsteed and Cassini's results. Indeed he deduced a value of 25° for the solar parallax from the Cayenne results. (In his various works Halley adopted various values of the solar parallax. He originally used 45°, reduced it to 25°, and finally published 12° in his Tables.) When the Abbé La Caille went to the Cape in 1751 he determined the parallax of Mars and deduced 10½° for the solar parallax. One should add of this method that in recent times, that is in 1924, Spencer Jones and Halm obtained the very good value of 8°.805 ± 0°.005, but in 1750 the results of the method were still considered rather unsatisfactory, the best value being about 10°.
I quote Cassini again, in his memoir of 1772.

Such is the point to which we had come, when the transit of Venus of 1761 provided for us the means of dissipating the residual uncertainty which we might have and deciding the question, either by confirming the results already found, or by rectifying it.

The method of determining the solar parallax by observing a transit of Venus across the Sun's disc was proposed by Halley in 1716, and described by him in the *Philosophical Transactions* of the Royal Society for that year.\(^1\)

Halley observed a transit of Mercury across the disc of the Sun, and considered that he was able to observe the moments of ingress and egress of the planet into and out of the disc with considerable precision. He realised that the apparent path of the planet across the solar disc depended on the observer's position on the Earth. Indeed, from a station near the South Pole the apparent line of transit would be north of the line as observed from the North Pole; and the angular distance between these two apparent lines would give the parallaxes sought. Much the best method of finding the distance between these apparent paths would be to measure the lengths of the two chords (provided that the two paths were well away from the centre of the Sun) and that could be done by timing the transits, the planet moving at a virtually uniform rate over the solar disc.

The circumstances are not very favourable for transits of Mercury, but they are favourable for transits of Venus; so Halley proposed, in 1716, that plans should be made to observe the next transit of Venus, which was unfortunately not expected to take place before 1761. This is because the orbit of Venus is quite highly inclined to the ecliptic (the plane containing the Sun and the Earth's orbit), the inclination being 3° 24', so that if Venus passes between the Sun and the Earth it will appear to pass above or below the Sun, unless the planet is rather near a node of its orbit (one of the two points where the orbit of Venus cuts the ecliptic), and the chance of Venus being sufficiently close to a node when it is in *inferior conjunction* (i.e. passes between the Sun and the Earth in longitude) is not very great. In fact it does so about twice in a century, and when this occurs a transit of Venus is seen. The transits usually occur in pairs, because the period of revolution of Venus about the Sun is nearly 8/13 of a year: so that if a transit occurs in, say, 1761 there will be one in 1769, which is 8 earth years and 13 Venus years later. But eight years after that the node will be too far away, and no transit will occur until the other node comes into the line of sight, which will be 113 1/2 earth years and 184 1/2 Venus years later. Transits in fact occurred in 1874 and 1882, in December,
THE SIGNIFICANCE OF THE TRANSIT OF VENUS

the transits of 1761 and 1769 being in June. (There will be another June pair in 2004 and 2012.)

This is only by way of explanation of the fact that transits of Venus occur very rarely, so that if the scientific world was to exploit Halley's method, great efforts had to be made to ensure that the transits of 1761 and 1769 were observed at stations widely dispersed over the globe, but especially in the north and south direction. Moreover, the longitudes of the observing stations had to be such that the event occurred by day, when it could be seen, and not in the middle of the night.

In the event the results of the expedition of 1761 were disappointing, and did not lead to a definitive determination of the solar parallax—at least, with the reductions which could be made at the time. This was partly because the spread in latitude achieved by observers who saw the whole transit—both ingress and egress—was limited, the most northerly of these stations being Tornea (at the head of the Gulf of Bothnia, 65° 51' N) and the southernmost being Calcutta (22° 33' N). The planned expedition to Bencooklen (3° 55' S) did not get there, and the expedition to Rodrigues (19° 41' S) was only partly successful. At the Cape of Good Hope (33° 55' S) only the egress was visible.

It is true that an observation of ingress at one station can be combined with an observation of egress at quite another station to give the required result, but this can only be done if the longitudes of the two stations are known accurately, and this was not the case in the eighteenth century. The eighteenth-century astronomers who wanted to find the solar parallax had to prefer observations at stations at which both ingress and egress were observed, so as to eliminate the effects of uncertainty in the longitude. A modern discussion of the observations can easily avoid this difficulty by supplying accurate longitudes, and this was done by Simon Newcomb in his definitive rediscussion of the observations, which he made in 1891, getting a good value of the solar parallax from the 1761 observations.

But this was not possible at the time: if they had to rely on stations at which both ingress and egress were observed, the eighteenth-century astronomers had only the arc from Sweden to Calcutta.

It is worth making a digression to inquire what went wrong with the plan to send an expedition to Sumatra. The Royal Society proposed to send a pair of observers who subsequently became rather well known, namely Mason and Dixon. Charles Mason was Bradley's assistant, and Jeremiah Dixon a surveyor and amateur astronomer from Durham. These two later surveyed the boundary between Maryland and Pennsylvania, which became known as the Mason and Dixon line, and Dixon's name has passed into immortality under the guise of Dixie in
the affectionate name, Dixieland, for the southern states. But in March 1760 these two set out for Sumatra from Portsmouth in H.M.S. Seahorse. When she was thirty leagues out of port, Seahorse was engaged in a violent battle with the French 34-gun frigate, le Grand. The cannonade left neither vessel the victor, but Captain Smith of the Seahorse lost eleven dead and thirty-seven wounded, and put into Plymouth.

The Admiralty was willing to refit Seahorse and escort her out of the Channel with a 70-gun ship of the line, so that she might proceed to her original destination. But Mason and Dixon had lost their enthusiasm. As Mason said, 'the uncommon misfortune that has attended our designs, and the sea sickness besides, have affected me in an Unusual Manner', and they both wrote to the Royal Society, trying to get the expedition switched to the Black Sea. However, the Society passed a very stiff resolution, saying that the refusal 'cannot fail to bring an incredible Scandal on their Character, and probably end in their utter Ruin', and indeed the Council informed Mason and Dixon that the Society would with the most inflexible resentment bring them into court and prosecute them with the utmost severity of the law. It was enough. On 3 February 1761 they wrote to the Society to say that their dutiful servants would depart that very evening, and so they did. But the harshness of the Society's strictness was softened by a postscript, which left it to the discretion of the voyagers, in view of the uncertainties of war and weather, to 'act in the best Manner, for Effectually answering the ends of their designation', and in fact they exercised this discretion by prudently stopping at the Cape, where they could observe egress only. In fact, Bencoolen had been taken by the French. Of course, had Western Australia existed, they should have gone to Perth. (The shortest duration of the transit was that occurring at Tobolsk; at Calcutta it was 3 minutes longer; at Bencoolen 4½; at Rodrigues, where ingress was not seen, 7½ minutes; but in 'New Holland', 10 minutes.)

James Short reduced the observations, including those made at Rodrigues and the Cape, using the best longitudes available to him. His second paper on the subject is printed in the Philosophical Transactions of 1763. He arrived at a solar parallax of about 8½ seconds of arc, but he had difficulty with the observations made by Pingré on Rodrigues, which he thought were one whole minute of time in error. Pingré asserted that it was impossible for him to have made an error of a whole minute in his time of contact, and we can now see that this is right, but that the longitude was wrong by a minute. Hornsby, the Savilian Professor at Oxford, published a paper in the same volume of the Transactions.
Hornsby found 8°.69 from comparing the Cape observations with those in other stations; and from Rodrigues, 8°.65 if he corrected the time by one minute, and 10°.4 if he did not. Since Hornsby's reduction of the intercomparison of the remaining stations leads to 9°.70, he was led to the speculation that Mason and Dixon were one minute of time wrong. But of course, it was again the adopted longitude of Rodrigues that was one minute wrong.

Cassini has a similar discussion.

This, then, was the difficulty in interpreting the observations of the transit of 1761. The observations at the Cape of Good Hope appeared to contradict those made at Rodrigues, and the arc of latitude described by the remaining observations was meagre. The solar parallax could have been 8½° if great weight was placed on the Cape, or 10° if weight was placed everywhere else. To reconcile these difficulties, suggestions were made that either Mason and Dixon at the Cape or Pingré at Rodrigues had timed their observations a whole minute wrong.

One can now see that it was the longitude of Rodrigues that was a whole minute in error; indeed, the eighteenth-century astronomers could have resolved their difficulties without waiting for the transit of 1769 if they had thought of redetermining the longitudes of the Cape and Rodrigues. How easy to appear omniscient two hundred years after the event; but what mistakes of ours will be so easy to explain in 2069, and more so in 2169!* As it was, it seemed vital to press forward with preparations to observe the transit of 1769 with all possible rigour.

To get the maximum benefit from the transit of 1769 meant that a station should be occupied somewhere in the south Pacific. A 'mappe-monde' was published by Lalande, in which the globe is divided into the areas in which ingress and egress can and cannot be seen. Ingress can be seen north of the line marked 'Entrée au coucher du Soleil' or 'Entrée au lever du Soleil'. Egress can be seen between the two lines marked 'Sortie au lever (coucher) du Soleil', that is in the Pacific generally. The area in which both ingress and egress can be seen runs from Murmansk through the north of Siberia to the Pacific, where it ends. In London, only ingress can be seen, and in India, only egress. All the times shown on Lalande's map are Paris Mean Times. The middle of the transit (from the centre of the Earth) occurred on 3 June 1769 at 10h 19m p.m. Greenwich Mean Time. (The convention used was that 0h occurred at noon.)

Arrangements were made on an international scale to observe the

* James Short used 4h 12m 42s E of Greenwich for the longitude of Rodrigues, and Cassini 4h 12m 52s E. But Simon Newcomb concludes that Pingré's station was 4h 13m 42s E of Greenwich.
transit from as many places as possible, but in order to extend the baseline southward it was necessary to occupy a station somewhere in the little-known southern Pacific; and this the Royal Society resolved to do. As is well known, the Society petitioned the King, it approved directions to be observed by Captain James Cook and Mr Charles Green with respect to their making astronomical observations, and the expedition, commanded by Captain Cook, sailed for the Pacific.

Cook described the astronomical observations made at Fort Venus in a paper in the *Philosophical Transactions* for 1771, entitled 'Observations made, by appointment of the Royal Society, at King George's Island in the South Sea; by Mr. Charles Green, formerly Assistant at the Royal Observatory at Greenwich and Lieut. James Cook, of His Majesty's Ship, the *Endeavour*'.

At Fort Venus they set up an astronomical clock by Shelton, and an astronomical quadrant of one foot radius by Bird, with which they observed the Sun, Moon, and Stars for time, latitude, and longitude. The published paper describes 'two reflecting telescopes of two feet focus each, made by the late Mr James Short, one of which was furnished with an object glass micrometer'. There was also a telescope of 3 ft focus which was used during the transit by Dr Solander.

The observations of latitude were straightforward enough. They gave a mean value of 17° 29' 15" S, which is substantially correct.

The observations of longitude are perhaps more interesting as an example of a very early application of the use of the *Nautical Almanac*, which, of course, first appeared in 1767. Both the methods of lunar distances and of observation of Jupiter's satellites were used, so that the determination of the longitude of Fort Venus represented a sort of culmination of the entire campaign to determine the longitude of very distant places by astronomical means, which started with the foundation of the Royal Observatory in 1675—that is, about a century before the appearance of the first *Almanac*—and the continuous observation of the Moon at Greenwich.

The mean of the lunar observations (fifteen of them) gave 149° 36' 38" W and the observations of Jupiter's satellites gave 149° 32' 30" W. The value adopted in 1891 by Simon Newcomb is 9° 17' 56" W, or 149° 29'.5 Taking this as correct, Cook and Green were in error by 8' and 3' in the two determinations, or by about 24 seconds of time in the mean. It is interesting to notice that no chronometers were taken on Cook's first voyage. He took two of them on the second voyage and was enthusiastic about their use; but he continued to take lunar observations in order to check his chronometers.

On the day of the transit, 3 June, they were lucky with the weather and observed both ingress and egress successfully. Both Cook and
THE SIGNIFICANCE OF THE TRANSIT OF VENUS

Green made sketches of what they saw, which were reproduced in the *Philosophical Transactions* paper; and they gave very detailed times of the contacts recorded. The detail is of very great importance because no observer really saw a sharp contact between the well-defined surfaces of the two spheres, and it had to be accepted that observers saw a ‘black drop’ develop between the limbs of the planet and the Sun, especially at internal contact.

Cook says:

The first appearance of Venus on the Sun was certainly only the penumbra and the contact of the limbs did not happen until several seconds after ... A faint light much weaker than the rest of the penumbra appeared to converge towards the point of contact but did not quite reach it ... at the total ingress, the thread of light made its appearance with an uncertainty of several seconds.

These phenomena were observed and recorded in some degree or another by many observers, and their precise interpretation remains uncertain. The ‘penumbra’ may even have been bad focus, as they had the hot sun playing on their mirrors in every case.

The results are summarised by Newcomb as follows, in Greenwich Mean Time.

<table>
<thead>
<tr>
<th></th>
<th>Green Phase</th>
<th>Cook Phase</th>
<th>Solander Phase</th>
</tr>
</thead>
<tbody>
<tr>
<td>I Ingress Exterior Contact</td>
<td>7 21 20</td>
<td>7 21 25</td>
<td>7 21 46</td>
</tr>
<tr>
<td>II Ingress Interior Contact</td>
<td>7 38 55</td>
<td>38 55</td>
<td>39 08</td>
</tr>
<tr>
<td>III Egress Interior Contact</td>
<td>13 9 46</td>
<td>13 9 56</td>
<td>39 42</td>
</tr>
<tr>
<td>IV Egress Exterior Contact</td>
<td>13 27 57</td>
<td>13 27 45</td>
<td>13 27 56</td>
</tr>
</tbody>
</table>

Newcomb’s classification of the phases is as follows:

The symbol $L$ signifies that the observer states that he observed the formation of a thread of light at ingress or its rupture at egress ... when the description seems to imply that the phase was the earliest formation of the thread of light ... [it] is called $L_1$. When the description seems to imply that the phase was appreciably later than the actual formation of the thread it is called $L_2$. $G$ indicates that the observer noted some phase earlier than $L$ at ingress or later than $L$ at egress.

(The difference between $L_3$ and $L_4$ must have been deliberate, but I am not sure what Newcomb meant by it.)

I have set the matter out in detail because the accuracy with which the duration of the transit could be measured was, of course, vital to the accuracy with which the solar parallax could be determined by
combining the Tahiti observations with those made at northern stations.

The exact interpretation of the differences between Cook, Green and Solander is quite vital if the solar parallax is to be determined from their observations without error of order 1 per cent.

An early interpretation was that of T. Hornsby. Hornsby’s discussion has the merit of extreme simplicity, and also the curious merit that it happens to lead to almost exactly the right result. He considered only five stations, at each of which both ingress and egress were observed, namely Wardhus, in northern Norway; Kola, near Murmansk; Hudson’s Bay; California; and King George’s Island. The station in California is interesting: it is called St José, but is not the modern San José near San Francisco; it is San José del Cabo on the extreme southern tip of the (Mexican) Californian peninsula. It was visited for the purpose by the Abbé Chappe d’Auteroche, who published his *Voyage en Californie* in Paris in 1772.

According to Cassini, the Abbé Chappe would have liked to have made his observation from the Solomon Islands, but he was unable to secure permission to do so from the Spanish Court ‘peu curieuse ordinairement de laisser les Étrangers prendre connoissance de ces mers’. (Captain Cook, of course, did not ask anybody’s leave.)

The Abbé Chappe d’Auteroche sailed from Cadiz on 21 December 1768, accompanied by two Spanish naval officers, Doz and Medina, and they arrived at Vera Cruz on 8 March 1769, after a voyage of seventy-seven days. They had some trouble getting into the harbour, as the vessel was French and the Captain refused to fly a Spanish flag under his French Standard; and when Chappe did at last get ashore, a hurricane blew for three days, causing him great anxiety lest the ship should part its cables and sink with his instruments aboard. But no disaster occurred; and on 18 March the party set out, in litters and on horseback, for Mexico City.

As a good Frenchman, the Abbé was distressed by the cuisine.

The Indians nourish themselves with a very poor bread made out of maize. They grind this corn, more or less, between two stones, damp the coarse flour which results with a little water, and make a paste which they flatten like a pancake, and cook it on a flat stone which they put in the middle of a great fire. These cakes are called *Tortillas* and are scarcely preferable to ship’s biscuit. As to the Ragouts with which the Indians regale themselves, they put in such a lot of pimento, and water them with such bad oil that it is impossible, especially for a Frenchman, to enjoy them.

After eight days they got to Mexico City, which was then, as now, on the shores of a lake, and built on marshy land. All the houses were
built on piles, and in places the ground had subsided. There were houses which had sunk six feet without collapsing, amongst which was the cathedral itself. This was not finished in 1769, and it was then thought unsafe to continue the building on account of the subsidence.

As they were now to journey through a country inhabited by wilder Indians, the Viceroy thought it necessary to provide an escort of soldiers.

Troops of wild Indians, whom the Spaniards call Indios Bravos, attack travellers if they think they are the stronger, and massacre them, or at least strip them and lash them to trees. They seize their baggage and their mules, which they take away to places only known to themselves, where they divide the money and hide the rest of the booty. There is a forest and a mountain, close to which we were due to pass, which our guides told us were full of treasures amassed by these brigands. These bandits could be recognised easily by a handkerchief which they grasp with their teeth in order to hide their faces. When one sees an Indian masked in this way come up to one the best thing is to strike first and kill him if possible. Happily, we did not have any of these bad encounters.

They left Mexico City on 30 March and got through to San Blas on 15 April, having taken twenty-eight days to cross Mexico. San Blas was 'a very small hamlet, situated on the West coast of Mexico, at the mouth of the river S. Pedro'. It was only a few years since it had been established for the purpose of transporting troops and provisions to California.

The passage from San Blas to Cape San Lucas is indeed only about 60 leagues, but the lack of wind and the adverse currents which one encounters in the Gulf often make the passage long and difficult. We had little enough time before the observation of June 3. Fortunately for us, on the same evening a Californian packet boat came into port, and it was at once placed at our disposal. We arranged to leave four days later.

The Captain of our vessel gave us little encouragement by saying that last year it had taken him 21 days to get from S. Blas to S. Lucas, although it was in a better season of the year than the present. I hesitated whether I had not better stay on the mainland, rather than risk finding myself at sea at the moment of observation: but I gave up this alternative as soon as I was assured that the rains began on this coast in the month of May and continued with practically no interruption to the end of the following month. The best choice was therefore to embark; so as to get to the other side of the Gulf and have much better chance of a good sky.

This choice was to ensure the Abbé a good observation, and the
world a better knowledge of the solar parallax. But it was to cost two lives.

They put to sea on 19 April; but it was as the Captain had said, and for fourteen days they had no wind and made no progress at all. Then they got a little wind which enabled them to go slowly north; on 9 May they were still off Matazlan on the mainland coast. The Abbé considered going ashore there: they were only one-third of the way from San Blas to Cape San Lucas, twenty days gone, and only twenty-eight days to go before the transit of Venus. But then the wind changed, and in another week, that is on 16 May, they saw the coast of California. On the 18th they were only five leagues from the shore. The Abbé, of course, wanted to go ashore wherever it was, and get going at all costs. The Spanish officers, Doz and Medina, counselled going another sixteen leagues northward to San Barnabé, where there was a better landing. The Abbé prevailed, and the Captain and his sailors put them ashore through the surf with great skill, with the Abbé sitting on top of his precious pendulum, to help protect it from salt water.

Here the Abbé's Journal stops, but not his astronomical notes. With immense energy he set up his pendulum and his telescopes, observed the latitude, observed the satellites of Jupiter to get the longitude, and observed the transit of Venus itself, all with complete success; and Doz and Medina did the same.

Nevertheless, tragedy struck. This is Cassini's account:

For some time a contagious illness had been raging in the village of S. Joseph which had already carried off a third of the inhabitants when M. Chappe arrived there. It might have been easy to get away from the place and establish a station nearer Cape S. Lucas, and this indeed the Spanish Officers proposed; but there were only a few days left before the observation, and a move would have cost precious moments. The Abbé was less sensible of the danger to his life than of the risk of losing his observation or making it incomplete; and announced that he would stay at S. Joseph.

Each day death reaped its harvest around the Abbé, and proclaimed the danger which he was courting; but each day the object of his desire drew nearer, and M. Chappe was insensible to all else. But the great joy which he felt in the complete fulfilment of his scientific aims was overshadowed by the desperate spectacle he was about to witness.

Two days after the transit, Doz and Medina and all the Spanish party to the number of eleven persons fell ill. The French assistants did the same. The Abbé was then the only man in good health. He had brought with him a medicine chest and a book, from which he was unable to determine whether he should purge or bleed his companions. (He should have done neither, but in fact he decided on purgatives.)
At length he succumbed to the fever himself. He rose from his sick bed on 18 June and observed an eclipse of the Moon (these observations were used in that century to help determine the longitude). One cannot, says Cassini, cast one's eyes on the details of this observation without wonder. One cannot conceive how M. Chappe, languishing, weighed down with suffering, weakened by the attacks that he had to fend off, was able to give to this observation all the attention which an observer in robust health could have given it. And indeed he could hardly manage it. He had attacks of fainting, and a headache which never left him; he actually persuaded his interpreter, not a surgeon, to bleed him in the middle of the observation. This only increased his illness.

For the next six weeks he got worse, and expired on 1 August. The party dragged itself back to the mainland, where Medina died, at San Blas.

Altogether, the transit of Venus of 1769 was responsible for four deaths, as Green died on the voyage home, and Veron, who accompanied Bougainville in his voyage round the world, also died on the voyage. Veron did not even have the satisfaction of observing the transit, as Bougainville's ship passed through the South Sea too soon.

There was another observer in California, whom Cassini refers to as ‘Velasque’ in the Voyage en Californie, but his observations are reported in such a way that Newcomb has to say (of the Voyage): ‘The same work contains a complete set of observations made at “Saint Anne” in California. I have not, however, succeeded in identifying the station.’

Velasquez was a Mexican-born official who would perhaps have been called a century later a District Officer, and was the Spanish Viceroy’s representative in lower California. As such he was responsible for the affairs of the French expedition, but as he was an amateur mathematician and astronomer he decided to make observations himself, and chose a station at some distance from Chappe in order to guard against the risk of all of the observers encountering cloud. So he set up a station on a hill from which he could see the sunset over the Pacific, at Santa Anna or ‘Sainte Anne, dans l’anse de Cerallo’, or, as Cassini says, ‘40 lieues au nord de San-Joseph en suivant la côte, on trouve le village de San-Anna, position qui n’etoit point rapportée sur les cartes’. Actually Velasquez determines his position as 110° 11’ W of Greenwich and 23° 39’ 55” N. Unfortunately Cassini does not mention this in the Voyage, and he quotes Velasquez’s times for all four contacts in such a way as to give his duration as two minutes longer than that of Chappe—and this, if it were correct, would place ‘Saint Anne’ in Arizona.

The original account of Velasquez’s work was found in the Naval Museum, Madrid, by Dr Iris Wilson, and a short account is published
One can now, I think, say why Newcomb, who cannot have known about the archives in Madrid, was so misled by Cassini as to lose Santa Anna. The story is as follows: Velasquez took to his hilltop a reflector of 22-inches focus by Short of London and also a Roman astronomical tube with two excellent well-ground lenses, and 5½ ft in length. Just before first contact the mounting of Short’s telescope became loose and Velasquez had to observe first contact with the tube. In the quarter-hour between first and second contact he mended the telescope, but he observed second contact with the tube, as he wanted to find the diameter of Venus by observing the two contacts with the same telescope, and observed with his spy-glass total immersion of Venus, or interior contact. But he then moved over to the telescope, and found that Venus had not yet completed its immersion, which, according to the telescope, was only completed at 12h 16m 11s—two minutes after the tube had given it. In the evening, Velasquez observed third and fourth contact with the telescope. Cassini quotes the times of first and second contact as measured by the tube and the times of third and fourth contact as observed in the telescope, and it is in this way that he makes Velasquez’s duration of the whole transit two minutes too long; as long, in fact, as it would be if seen from Arizona. This is why Newcomb could not place Santa Anna; and if one takes only the telescope observations, Velasquez is 30s late, 55s late, and 7s late at second, third, and fourth contacts—which is a very respectable showing. This is assuming his own determination of his longitude, and a small change would make his transit observation still better.

To come back to the solar parallax, Hornsby rejected Green’s observation of the ingress and took the mean of Cook’s and Green’s observations of the egress—and gave no weight to Solander. The real reason for Hornsby’s selection of the Tahiti observations is that he chose the combination which made the shortest duration of the transit as seen in Tahiti, and therefore the one which brought the observations best into line with those made from the shorter arcs on the Earth’s surface. After producing his result, which was 8°.78, Hornsby remarks that ‘the learned of the present time may congratulate themselves on obtaining as accurate a determination of the Sun’s distance, as perhaps the nature of the subject will admit’. As the value found by himself from the 1761 transit of Venus was 8°.69, assuming Mason and Dixon to be right, he had some reason to regard the subject as closed. And indeed the matter looked rather satisfactory. Kepler supposed the solar parallax to be not less than 59 seconds, and Hevelius, 41 seconds, whereas Flamsteed, from observations of Mars, supposed the parallax not to exceed 10 seconds, and Halley, ‘in a memoir written expressly with a view to
ascertaining the exact quantity of it, supposes it to be not greater than 12 ½ seconds. Now Hornsby seemed to have pinpointed it, but of course the subject could not be allowed to rest as Hornsby had left it. In all, more than a hundred observers had timed either ingress or egress or both; and observations of ingress alone, or of egress alone, could be combined with other observations, if the longitudes of the observing stations were known. Many of these were, of course, uncertain in the eighteenth century, but by the middle of the nineteenth century the longitudes of nearly all the stations could be given with accuracy, and in 1835 J. F. Encke published a comprehensive discussion of the material. He found the very low value of 8".577, which conflicted with careful determinations made later in the century by P. A. Hansen (from the parallactic equation of the Moon), L. Foucault (from the velocity of light), and E. J. Stone and A. Winnecke (from observations of Mars), all of which gave values between 8".86 (Foucault) and 8".96 (Winnecke). As Newcomb says, the result of these investigations was a general tendency to assign to the solar parallax between 8".90 and 9".00, entirely rejecting Encke’s result, that is to say paying no attention to the transits of Venus.

Newcomb considered ‘that we should not consign to oblivion a mass of material so celebrated in the history of science’ as the observations made upon the transits of Venus in 1761 and 1769, and embarked upon an extremely thorough and penetrating analysis of them which he published in 1891. Newcomb scrutinised the original accounts of the observations and in many cases reproduced the observers’ own descriptions of the phenomena, with the special object of developing an adequate treatment of ‘the threads of light’ and ‘black drops’ seen between the planet and the limb of the Sun. He weighed carefully every account, and rejected as erroneous a number of observations. Even so, no fewer than fifty-eight observations of the fourth contact of 1761 and forty-five of the third contact of 1769 survived.

Of San José, Newcomb says: ‘I suppose the mean of the times given by the other two astronomers to be entitled to the same weight as that of Chappe’, and for Tahiti, he gave half weight to Solander but equal weight to Cook and Green—removing the arbitrary selection exercised by Hornsby.

Newcomb dealt with the phenomena of threads and black drops by supplying a series of corrections which were multiples of an error which he denoted by $k$. He says:

let us represent by the symbol $c$ the amount by which Venus must impinge upon the Sun in order to be visible under the best conditions, that is under conditions when the coefficient in question is zero. Let
$c + k$ represent the thickness of the thread of light corresponding to the coefficient unity; then $c + 2k$ will represent the thickness for coefficient 2, etc.

Newcomb assigns coefficients from 0 to 4 according to the Sun's altitude, the focal length of the telescope, and the statements of the observers themselves.

Since the coefficients of $k$ are various, the error $k$ enters into the equations of condition in such a way that it can actually be determined by the method of least squares, and Newcomb allowed the least squares solution to determine it in each case. He expected it to be always positive, but nearly zero in the case of interior contact at egress, when the observer had the best chance of seeing exactly what happened, and timing it properly.

His results for 1769 are:

I Ingress, exterior contact $k_1 = +0''.20 \pm 0''.06$

II interior $k_2 = +0''.04 \pm 0''.04$

III Egress, interior contact $k_3 = -0''.05 \pm 0''.08$

IV exterior $k_4 = +0''.20 \pm 0''.08$

and he got very similar results for 1761. The incorporation of this device leads to the results for the solar parallax:

\begin{align*}
1761 & \quad 8''.77 \quad \text{weight 11} \\
1769 & \quad 8''.79 \quad 33
\end{align*}

The weighted mean is in fact $8''.79 \pm 0''.051$ s.e.

This value, of course, agrees very well with the most modern astronomical determinations, which are as follows:

A. R. Hinks\textsuperscript{17} from observation of Eros in 1900-1 $8''.807 \pm 0''.003$

H. Spencer Jones\textsuperscript{18} from observation of Eros in 1931 $8''.790 \pm 0''.001$

The best determinations are undoubtedly those from radio observations of echoes from Venus, of which there have been several. Allen, in Astrophysical Quantities, gives the best value as

\[8''.79415 \pm 0''.00005.\]

So Hornsby was right after all. To quote Newcomb again:

The mutations of opinion on the value of the observations made upon the transits of Venus in 1761 and 1769 are noteworthy. For more than a century these observations were looked upon as affording the best data for the determination of the solar parallax, and the future epochs of such transits were anticipated as the only times when valuable additions to our knowledge of distances in the solar system could be made. Now, however, opinion has changed so far in the other direction.
that some apology may be needed for devoting the time and labor which I have to the present discussion.

Newcomb's labours were not in vain; nor were those of the intrepid voyagers like Cook, and the deaths of the martyrs Chappe, Green, Medina, and Veron. After all, the astronomers of the early nineteenth century knew the distance to the Sun much better than present-day astronomers know the distance to the centre of the galaxy.
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