ALISTAIR CROMBIE

# Alistair Cameron Crombie
# 1915–1996

ALISTAIR CAMERON CROMBIE, one of the most influential historians of science of his generation, died at his home in Oxford on 9 February 1996. He was born on 4 November 1915 in Brisbane, Australia, the second son of William David Crombie and Janet Wilmina (*neé* Macdonald), both of Scottish extraction. His grandparents had made the move from farms in Scotland to very remote properties in Queensland, where they raised sheep in truly pioneering circumstances. From the beginning they were heavily dependent on the vagaries of the weather, and there is one family story of how their fate on the original trek had hung in the balance, depending entirely on their success in locating a river, which they managed to do with very little time to spare. By the time Alistair Crombie was born, the sheep stations were prospering. After school at Geelong Grammar School, he began his university career at Trinity College, Melbourne University, as a medical student, and took his first degree there in zoology in 1938. Most of his adult life, however, was spent in England, and more than half of it in Oxford. Leaving Melbourne, he continued his studies at Jesus College, Cambridge, where he took his doctorate in 1942 with a dissertation on population dynamics—a fact that explains his lifelong interest in the history of Darwinism. In a survey of the history of insect ecology by H. G. Andrewaka and L. C. Birch, his zoological work from this period is said to have greatly stimulated other research on interspecific competition. Between 1941 and 1946 Crombie occupied a temporary research position with the Ministry of Agriculture and Fisheries, which entailed working in the Cambridge Zoological Laboratory. His scientific interests led him to follow informally the lectures of the philosopher C. D. Broad, whose book *The Mind and its Place in Nature* greatly influenced Crombie. He was also

*Proceedings of the British Academy,* **97**, 257–270. 

inspired by a reading of R. C. Collingwood. Steadily he moved in the direction of the philosophy of science and from there to the history of science.

## University College London

In 1946 Crombie was appointed lecturer at University College London. Institutionally, at least, UCL had long been the main centre in Britain for the teaching of the history and philosophy of science, although not at an undergraduate level. Abraham Wolf had been the main driving force, immediately after the First World War, and an M.Sc. degree in 'Principles, History and Method of Science' was established there as early as 1924. Following the end of the Second World War, the astrophysicist and writer on philosophy of science Herbert Dingle was made head of the department, and in 1946 Crombie was recruited as one of its staff of five. The way in which the subject was taught at that time can be easily judged through Wolf's writings and those of Charles Singer (who was at UCL for a brief period from 1919), and their successors. (Douglas McKie, Angus Armitage, Niels Heathcote, and Dingle himself were Crombie's colleagues.) Broadly speaking, the subject was seen as the narrative history of successful scientific ideas and invention, and was meant to answer to the needs, or at least the interests, of the science departments. This is not to say that standard scientific criteria of acceptability were always enough. There were various scientific subjects that Dingle, who was carrying on a running battle with the relativists, would not countenance as a proper object of study, but this did not affect Crombie's teaching. Cambridge experience (and he continued to live in Cambridge) had made him more comfortable with the methodologists than the chroniclers, but above all it led him to view his subject as essentially a branch of the humanities. He was at least one with his new colleagues when in 1947 he joined with them and others in setting up the British Society for the History of Science as well as its Philosophy of Science Group. The latter became an independent body, the British Society for the Philosophy of Science, and Crombie became in fact the first editor of The British Journal for the Philosophy of Science, but by degrees he was moving in the direction of history, albeit of a sort very different from that in vogue at UCL.

UCL's influence on this dual subject should not be underestimated. In the years between the wars more than a hundred higher degrees were taken in the department there. One of those who had taken the doctorate was Frank Sherwood Taylor, who after Robert Gunther's death in 1940 became for a decade curator

of the 'Old Ashmolean Museum'[1] at Oxford. Oxford had seen a number of half-hearted attempts over the previous thirty years to launch courses and research in the history of science beyond classical (natural) philosophy — most actively in astronomy and medicine — and Sherwood Taylor tried yet again. Courses were offered (in 1947–8) by S. F. Mason (an assistant demonstrator, in the parlance of Oxford science) and also by Crombie, as a visiting lecturer. Thwarted in a number of ways, Taylor left for the directorship of the Kensington Science Museum, to be succeeded by Conrad Josten in 1950. Josten inherited the ambition to institute teaching courses, but as a director rather than as a teacher. (His quietly imperious style did not always achieve the desired result. On one occasion it led the museum technician to clean one of the two blackboards preserved religiously from the time of a visit by Einstein to Oxford in 1931.) As a result of an initiative by Josten and others, a lecturership in the history of science was created in 1953, and Crombie was the successful applicant. His task was to be that of establishing the history of science as a normal part of teaching and research for students of science, history and philosophy, but under the control of the Committee of the Museum.

By 1953 Crombie had been working for some years on a study of various aspects of medieval science, and he had written two important books in parallel, one a general text-book, the other a scholarly monograph. The first, *Augustine to Galileo: The History of Science A.D. 400–1650* (London, 1952, expanded and modified in small but important ways in 1959), filled a significant void.[2] Eventually translated into seven languages, over the next thirty years it became one of the world's most widely used text-books of history of science. His other early work, *Robert Grosseteste and the Origins of Experimental Science, 1100–1700* (Oxford, 1953), grew out of an essay he wrote for the seventh centenary of Grosseteste's death, an occasion for other studies, written by more experienced scholars but from more traditional points of view. While Crombie was primarily concerned with Grosseteste's scientific thought, and ignored many of the things that had made the great bishop seem so important to other historians, he touched on a number of much more general themes, analysing the whole question of continuity and change in the European scientific tradition from the middle ages to the seventeenth century. Crombie's

[1] This was the common name for the Museum of the History of Science, an institution that had grown out of a collection of early scientific instruments assembled by Lewis Evans that was housed in the old Ashmolean building in Broad Street. The name was used by Robert Gunther, the first curator of the museum, to the intense annoyance of Lewis's brother, the archaeologist Arthur Evans, whose affinities were with the Ashmolean institution.

[2] An interesting account of how he changed his stance between editions will be found in B. S. Eastwood, *Isis* (March 1992). For an almost complete bibliography of his writings, see J. D. North & J. J. Roche, *The Light of Nature. Essays in the History and Philosophy of Science Presented to A. C. Crombie* (Dordrecht, 1985), supplemented by his own *Styles of Scientific Thinking* (London, 1994), discussed below.

claim to be able to detect in Grosseteste's thought significant traces of the experimental philosophy that made the scientific revolution of the seventeenth century possible has since been the object of much criticism. He overstated his case, perhaps, but much of his argument can be retained, and there is no doubt that the parts that are unacceptable served an important catalytic purpose in the decades that followed. His work was at least utterly different in character from that produced in the milieu in which he had worked up to that time.

## Oxford

Crombie moved to Oxford in 1954, after a year as visiting Professor at the University of Washington, Seattle—a diplomatic error on his part, since there was much good will towards his subject in the university, but also a sense that matters ought to be moving more quickly. Indeed, there had been for some years a sporadic discussion of the merits of establishing a 'science Greats' analogous to 'classical Greats' (Literae Humaniores) and to 'modern Greats' (a name occasionally given to the course in Philosophy, Politics, and Economics). I first learned of the ruffled feathers quite fortuitously at approximately the time of the Crombie appointment, when as an undergraduate I met a very taciturn physics professor, holed up in a corner at a party given by the wife of the Warden of Merton. This turned out to be Frederick Lindemann, soon to be made Viscount Cherwell, of whose name and reputation I then knew absolutely nothing. From near silence he was suddenly galvanised into speech when I mentioned a point in the history of physics. He produced a succession of sharp personal remarks that meant nothing to me, but I was intrigued at the idea that such a dull subject could produce such an animated response. Since I had just visited the 'Old Ashmolean' I was also amused at his judgement that the museum was living proof of the law of increasing entropy.[3] But what I recall most clearly was his insistence that the history and philosophy of science—of which he seemed to have no high opinion—were the property of the scientists. I later heard this said often in Oxford, and it represented one of the hurdles that Crombie would have to cross.[4]

[3] As I later discovered, he was wrong, for C. H. Josten had by this time turned the graph downwards, but the effects had not yet made themselves felt.

[4] The point was made strongly in a letter to *The Times* on 17 August 1956 by a certain I. Aucken, who suggested that no one with experience of the tedium and delight of science will surrender this work for the sake of writing about other people's work, and yet that no one who lacks the experience is 'competent to write history that is safe for serious students of science to treat seriously'. To unseat himself from the horns of his dilemma, he conceded that the musings of superannuated scientists might have a genuine value. Such rhetoric is now much harder to find. The correspondence from which this item comes (see the issues between 11 and 25 August) included contributions by Crombie as well as Toulmin, Marie Boas, Dingle, and others.

When Crombie arrived, Stephen Toulmin was university lecturer in philosophy of science (he was there between 1949 and 1955), but most philosophers still felt very wary of these subjects. The point was put very well in an eloquent radio talk by J. P. Corbett, late in 1956, when he said that it would be better to draw a veil of silence over the questions on the methods and foundations of science in the so-called logic papers of the Oxford schools, but that it was high time that those responsible for the Oxford curriculum recognise that science is a significant strand in the social process. (More recognition within philosophy came soon afterwards.) Both Toulmin and his successor Friedrich Waismann included a historical dimension in their teaching, but to all intents and purposes Crombie was building on very low foundations. As for the history faculty, there was then very little systematic interest within it, beyond an occasional excursion on the part of individuals who found themselves unable to ignore entirely the scientific aspect of past intellectual life; but those individuals tended to feel very strongly about their subjects—and Grosseteste is only one of many shared subjects that come to mind—and did not like intruders. In short, the battle lines with which Crombie would have to contend were drawn in many Oxford minds before he ever arrived on the scene, and perhaps it was his failure to understand them that gave him something of a reputation for high-handedness.

I first met him personally soon after he arrived. I was an undergraduate and he had been persuaded to teach a course on the Leibniz–Clarke Correspondence and parts of Newton's *Principia* for one of the papers in philosophy. Already he was in the thick of controversy that stemmed in large measure—but of course not entirely—from proprietorial imperatives. His services were being called for by people in different faculties, but in the tradition of Oxbridge democracy he regarded himself as the guardian not only of his own conscience but of his time. Josten, more familiar with a continental hierarchy, had other ideas, especially when it came to the facilities of the museum.

The Leibniz–Clarke class, held at first in a room in the Old Ashmolean building, helped to bring matters to a head, but it was memorable for quite different reasons. It was the beginning of what became an extremely influential seminar in the history of science generally, not so much by virtue of Crombie's own contributions but by his guidance and his ability to attract notable senior scholars, many of them visiting Oxford from elsewhere. When Josten decided that coexistence was not possible, the meetings moved to All Souls, where Crombie was a member of common room. After 1969, when he was made a fellow of Trinity, and until 1983 when he retired, it was held in Trinity, and for all of these years it was the centre of gravity of Oxford history of science, at least in the eyes of most of the world outside Oxford. The meetings were often very casual, but they were not trivial. They had a vital core that will not

emerge from a mere scanning of lecture lists and committee minutes, and the vitality came in part from the expectations of those present. The fortunes of the sciences themselves were burgeoning, and Crombie's moderate imperialism was such that as he became a talking point so did his subject. And slowly a feeling was growing that here, running in harness with philosophy of science, was an important subject that did not need to cling to the coat-tails of the sciences, one that had something to say to people from many other quarters of the university.

This is not to overlook the scepticism that was being expressed by some scientists in the university, almost certainly a majority, whose charge was that they were being asked to accept a soft option. They could point to the fact that much of the British literature of the period was of a very indifferent quality. The best work of the first half of the century was done by amateurs of some stature, such as Thomas Heath, J. K. Fotheringham, Edmund Whittaker, and Joseph Needham, but there was much over which it would be kindest to draw a veil. In a statement made at an Oxford conference in 1961 that amounted to his vision of the subject at that time, Crombie was obviously very sensitive to this charge:

> It seems difficult to see how a prospective historian at least of modern science can become really confident of his technical mastery of his materials unless he has had some fairly advanced training . . . in, and experience of the actual *use of* the analytical disciplines of science or mathematics, as distinct from simply studying the historical sources.[5]

This sentiment was well received by his select and distinguished international audience, and yet, nearer home, it was one that some in the history faculty were beginning to view with suspicion. It is with wry amusement that one can look back on a slow transition that was beginning to take place from a situation where scientific knowledge was thought to be not absolutely necessary to one in which it would be absolutely unnecessary. This might have mattered less had the history faculty not bid fair to corner the market, and to dictate the ground rules of the game into which it had entered late, but fresh. From another quarter came others preaching sociological correctives to more traditional intellectual history. Oxford conservatism managed at first to keep the wilder excesses of this movement at arm's length, but it did eventually have its repercussions.

For Crombie on his arrival this was all in the future. From the first, his style showed how he had been much influenced by Alexandre Koyré—one of several notable early contributors to the Oxford seminar, and one who did

[5] He went on of course to say that this was not enough. See 'History and Philosophy of Science at Oxford', *History of Science*, 1 (1962), 57–61, at 58. The paper summarised a talk given at a conference he and R. Harré hosted in Oxford in 1961, the proceedings of which were published in *Scientific Change: Historical Studies in the Intellectual, Social & Technical Conditions for Scientific Discovery & Technical Invention* (London, 1963).

Crombie the honour of disagreeing with him in a constructive manner. Crombie learned much from Koyré's criticism of his arguments for the influence of medieval thought on early modern science, as presented in his *Robert Grosseteste*.[6] Koyré's characteristic historico-philosophical outlook was not uncommon on the continent at that time, and indeed, an influential work by the American scholar E. A. Burtt had been written in a rather similar style even earlier.[7] Crombie, like the majority of those who attended his regular seminars before the mid-1960s, followed suit, treating history and philosophy as natural partners.[8] This approach struck a chord in the small but loyal group that came fairly regularly from the science area. As matters turned out, the formula did not last long. Gradually new forces entered the equation, and Crombie's career bore all the marks of these.

The ambitions that had been responsible for the rapid growth of the history and philosophy of science as a joint enterprise in the 1950s, both in Britain and abroad, were fading in the face of high specialisation on the part of the very people who should have been teaching across a broad spectrum.[9] As a guarantee of his independence, Crombie had engineered the setting up of a committee for History and Philosophy of Science independent of the museum, and through it—and through the good offices of such figures as William Kneale, Gilbert Ryle, Maurice Bowra, John Austin, William Paton, Friedrich Waissmann, and his successor Rom Harré—he was eventually able to introduce the joint subject into various corners of the undergraduate curriculum of five different faculties, as well as into the tough B.Phil. course, and to institute a Diploma, conceived as preparatory to doctoral work. The very breadth of his vision, which also characterised much of his writing, was not politically advantageous. These were the years of the Two Cultures discussion, but Crombie soon learned how empty was so much that was said in favour of bridges between the two. Again, the proprietorial compulsions of certain senior colleagues stood in his way—it was harder to claim territory that was conceived to lie on both sides of the bridge—and the old allies drifted apart. There was also unease among certain graduate students, who wished to specialise rather than to be forcefully broadened. (History graduates, for example, would typically argue that to have added the history of seventeenth-century science

[6] A. Koyré, 'Les Origines de la science moderne', *Diogène*, Oct. 1956, no. 16, 3–31.

[7] E. A. Burtt, *The Metaphysical Foundations of Modern Physical Science: A Historical and Critical Essay*, 2nd edn. (London, 1932).

[8] Koyré's influence on him was significant, but it has to be said that in his later work on Galileo Crombie resolutely opposed many of Koyré's theses concerning the platonising rationalism of Galileo.

[9] This lamentable narrowness becomes serious in the context of undergraduate teaching. There is nothing like the school curriculum in regular history to force breadth on historians of science.

to their repertoire was broadening enough, without further involvement in the science itself or philosophy of science.)

Another problem was that Crombie's broad outlook did not always fit very comfortably with the needs of the honour schools, the key to Oxford academic politics. Lecturing was not his forte, and he did not think tutoring to be his function. The undergraduate courses set up for the historians and for students in the natural sciences were taught independently, and by very different types of tutor. The scientists, the historians and the philosophers all had different interests and expectations, as he well realised, but there was more at stake than the notion of a single unifying discipline. He could not find a formula that would hold the allegiance of all interested senior members of the university whose support he badly needed, let alone the uninterested—such as D. N. Chester, the Warden of Nuffield, who made a sharp attack in Congregation in which the strongest argument against the subject was that it was administratively very inconvenient. Had Crombie taken a narrower view of his subject, and retreated into a scholarly corner, he might have avoided the common complaint that he was empire-building. He was not a good judge of the impression his plans were making on his colleagues, or of their true intentions. When a few Diploma students made known their displeasure at the idea that they should study philosophy of science, the situation was seized upon by some of his colleagues to embarrass him. There were even those who claimed that he was part of a Catholic plot to infiltrate the curriculum. (Ernest Jacob was jovial but serious when he said this on one occasion within my hearing.) At the other extreme of academic crustiness there were those who complained that Crombie was being 'irrelevant' in ignoring the sociology of science—a bandwagon that was gathering momentum in the late 1960s. The era of the atom bomb produced a curious ambivalence—a certain reluctance to admire the scientific mentality that had produced it, combined with a feeling that at least something must be said about it. By a strange sort of logic, some managed to proceed to the thesis that scientific mentalities were not a proper object of study except through their external repercussions. Their inner workings were deemed socially irrelevant, and therefore the study of them somehow violated the canons of good history.

Crombie was bemused by much of what was happening around him, and was content with the focus he had created for research at an advanced (rather than an undergraduate) level, largely unaware that others had plans for the subject differing from his. He had created a focus for academic controversy of perhaps too many different sorts, and this eventually worked against him. By the time the Oxford chair in history of science was created and filled (1971–2), the number of graduate students pursuing the history of science at Oxford had grown to more than fifty—of course by no means all were working with him—and the number of science undergraduates reading the special subject annually had peaked at over thirty. Despite this, the part he had played in

building up the subject counted for little in the eyes of a committee whose members had a more parochial view of the field than his, and in some instances harboured a measure of deep personal animosity. It was to the great surprise of many outsiders that he was passed over for the chair. The fact certainly took away much of his missionary fervour, but he continued to act as a magnet to scholars from elsewhere. He was known either personally or through his writings to a very large number of scholars, and this, and the fact that he had been so active internationally, meant that the subject in Oxford continued to be identified more closely with him than with any other single person.

## Styles of Scientific Thinking. Galileo

As a historian of science, Crombie's central interest was in the methods and styles of scientific thinking and reasoning and their development within the intellectual context of medieval and early modern Europe. It was a theme to which he returned repeatedly, and he spent more than twenty years in expanding what began as an article on the subject into a monumental three-volume study: *Styles of Scientific Thinking in the European Tradition: The history of argument and explanation especially in the mathematical and biomedical sciences and arts*.[10] In this he made a detailed comparative analysis of the forms of scientific reasoning that were developed within European intellectual culture, beginning with the Greek search for the principles of nature and argument itself, and applied to an ever wider variety of subject-matters. He thought that those who are concerned with the history of European scientific thought as a whole were obliged to begin there, since this very phenomenon was one initiated by the ancient Greeks. He went on to develop his strongly Eurocentric thesis into a demonstration of the importance of six styles of reasoning that were a characteristically European production. The styles in question had often been studied by philosophers of science, but Crombie's historical work was new both in its comprehensiveness and its thoroughness. In it he makes his reader aware—often perhaps unconsciously—of how little was the attention paid by so many modern historical writers to either the content of science or to the principles either of nature or of argument.

His six styles are: *postulation*, as used in Greek mathematics and the mathematical arts; the *experimental strategy* in the search for principles; *hypothetical modelling*, with its use of analogy; *taxonomy*, as the logic of ordering agreement and difference, and as a tool in the search for natural affinities and systems; *probabilistic and statistical analysis*, as a guide to reasonable expectation; and finally, *historical derivation*. By this last, he refers

[10] London, 1994.

to the genetic method, the way of looking at ostensibly unlike things and deciding on the grounds of their common characteristics what common 'historical' sources they have—a method applicable in subjects as diverse as linguistics and biology, as geology and the history of science itself.

Crombie's thesis was not without its problems. Beginning with postulation and the ancient search for principles and methods, he could not sidestep Mesopotamian and Egyptian science, in other words, science that was in no sense European. His way with this difficulty was to stress the importance of systems of thought. 'There is nothing', he wrote, 'in any surviving text corresponding to a theorem or a proof, no theory of numbers or generalized algebra, nothing that might indicate even an inkling of such conceptions.' In short, he believed that in Mesopotamian and Egyptian astronomy, medicine, and other empirical subjects, 'a general theory, a generalized explanation, a conception of natural causation' was lacking. He claimed that these cultures paid excessive attention to superhuman beings whom they credited with responsibility for phenomena, beings with a fickle character out of keeping with the regularities that guided European science.

His second style of scientific thinking he illustrated with reference to the science he had spent most of his life studying, from the thirteenth century to the seventeenth, a period that saw the advent of a sophisticated logic of experimental argument in different forms. The period saw a vigorous continuation of ancient discussions, but there was also a dialogue between natural philosophy and new developments in the practical arts, a dialogue that was later reinforced by ancient texts, and finally transformed into the art of the rational experimenter. In view of Crombie's well-known researches into Galileo, it comes as no surprise to experience a crescendo at the end of the first volume and the beginning of the second, when Galileo comes on stage. This is a cue for philosophical strategy to be reviewed in the light of the debate on Copernicanism, the character of the book of nature, the distinction of primary and secondary qualities, and so forth. Galileo is in fact kept securely in his place, a bright beacon, but one with a historical meaning only in relation to a network of others—a list of which includes Averroes, Clavius, Kepler, Ramus, and the Collegio Romano, and later takes in the Royal Society, Boyle, Newton, and Fontenelle. Mechanics, painting and sculpture, and the science of music are all fitted into this 'style', although they surface again under the next, that of 'hypothetical modelling'.

The modelling of natural processes—which has attracted much attention among philosophers of science during the last thirty years—Crombie presents as a technique introduced into early modern science with the help of ancient and medieval theology and the medieval and Renaissance arts. His examples include the modelling of the senses, with the camera obscura and the eye, as discussed by Leonardo and others. But there are analogies both grander and

deeper. There is that all-pervasive analogy between divine and human creation, and there is also what might be counted a second-order analogy, between making and knowing—and here Crombie has interesting things to say about the modelling of language, a historically important theme.

The taxonomic style gets the shortest treatment. Crombie follows its history from the Hippocratic writings to the eighteenth century, in fact to Michel Adanson and Buffon, who gave a refreshingly modern answer to the question of the reality of species. They were not essences, said Buffon, but a series of individuals defined by genetic continuity. Those are of the same species that have the power to reproduce their likeness. That of course leaves the question of explicating likeness, so that essentialists are not exactly forced into retirement. When this section of the book ends, however, Darwin is on the horizon, and the cruder forms of essentialism are becoming a backwater—for if individuals are the only reality, then variations give evidence that living organisms can change from generation to generation, and form new species.

Probabilistic styles of scientific thinking, fifth in the Crombie series, are styles that arise out of a need to formalise decision-taking in the face of uncertainty. They have their roots in Greek and medieval qualitative notions. Crombie starts from the Hippocratic writings once more, although he acknowledges that this style of reasoning was not well quantified or axiomatised until the sixteenth and seventeenth centuries. The names to conjure with now include Pascal and Huygens, Leibniz and Halley, Bayes, the Bernoullis and Maupertuis, d'Alembert and Laplace. From the need to gamble rationally—whether at cards or on a patient's life-expectancy—to the biology of populations is a long intellectual journey, or rather only the beginning of a longer one, and it passes through some extraordinarily rich territory. Fate, the will of God, divination, the presentiment and knowledge of future events, scientific determinism, mercantile insurance, the morality of gambling in relation to covert knowledge of certainties, and economic and demographic statistics: these are only a beginning. The core natural sciences are not perhaps at first conspicuous, but they are never far away. Chance and providence become changed into principles of least action, not only in physics but in the adaptive diversification of living things. Malthus, for example, having (as he believed) modelled his notorious argument on Newton, was at length taken over by Darwin and Wallace when they used statistical methods to analyse evolutionary change. Heisenberg, Schrödinger, and other moderns fell outside the terms of Crombie's history, but that they too can be brought into the picture must be in the mind of many a reader. This was not unexplored territory, but what Crombie achieved was its integration into the map of western science generally, something that had not been done in quite the same way before.

His last style was that of historical derivation, again traced by him to a characteristically Greek point of view, that the world is generated by a natural causal process that continues to operate. Taking a class of phenomena and trying

to identify common properties or common causes, one is led to the construction of a common ancestor. Crombie had much to say of the ways in which the method was applied in the sixteenth century with great success to the phenomena of language, but even more about the seventeenth- and eighteenth-century writers, say from Francis Bacon to Monboddo, who used the method to elucidate the history of the human mind and its mastery over nature. It is not just a question of postulating a common source: one must also be in a position to put forward an account of divergence from that source. One of the best chapters concerns the 'history of nature' — the geological history of the Earth, the origin of fossils, the explanation of the diversity of living types, and ultimately the transformation of species. Crombie was most at home in the life sciences, and those passages dealing with evolution and its antecedents are a pleasure to read.

This sixth brought him round once more to his previous 'style', for the transformation of organisms through time might be done not only by an innate, inbuilt, principle, but also in the style of a Maupertuis or a Laplace, by the statistical accumulation of marginal advantages. And so to Malthus' argument and its consequences, to which both Darwin and Wallace acknowledged their debt for the statistical treatment of natural selection, the model of net marginal advantage to the biology of populations. This was the subject of Crombie's last chapter. As he wrote:

> Taking a principle from the social sciences, they tried to demonstrate in the style of theoretical physics that its operation must, by an automatic statistical necessity, bring about increasing order from unordered random variations. The accumulation of repeated survivals of inherited advantages and disadvantages at different rates must necessarily transform and diversify the successive generations of competing automata to fit their diverse opportunities in the economy of nature, so that the better adapted must multiply, evolve and inhabit the Earth.

This well-received three-volume distillation of his life's work and ideals revealed among other things Crombie's abiding interest in the history of theories of the senses — echoing his earlier work in biology — and in particular the physiology and epistemology of vision and hearing, and their relation to the visual and musical arts. These are subjects touched upon in his numerous other publications — for example in the three published volumes of his collected essays — but they run through another of Crombie's studies, one that occupied much of the last thirty years of his life. Awarded the Galileo Prize by the Domus Galileana in Pisa in 1964, for an essay on Galileo, Crombie soon became a leading authority on that crucially important figure. His Galileo interest made Oxford a natural place to set up the Harriot Seminar, something we did with the collaboration of several distinguished Harriot scholars and the financial support of Dr Cicely Tanner, in 1967.[11]

[11] Between 1990 and 1997 Harriot continued to be celebrated in Oxford through an annual lecture in Oriel College. The pattern of the original Oxford seminar is followed more or less at Durham and Cambridge.

Crombie worked for more than three decades on two other books, *Galileo's Arguments and Disputes in Natural Philosophy* and *Marin Mersenne: Science, Music and Language*. His final illness, a brain tumour, took him unawares, and they were not to be. The second of these works is effectively to be found dispersed through his published writings, and much of the former can also be found elsewhere, although a typescript survives, awaiting additions by a collaborator, Adriano Carrugo. All of these component writings are true to the ideals of his formative years, in that they treat of science as a rational and not merely a social activity. The Galileo book led along the way to much invigorating controversy, largely over priority in the discovery and interpretation of key documents concerning Galileo's intellectual development, so that Crombie's main theses are well known to Galilean experts.

## Recognition

Alistair Crombie was much more than an author. He did much to organise his subject, nationally and internationally. Apart from editorship of the BJPS he was one of three joint founders (with Mary Hesse and Michael Hoskin) and for some years joint editor of the review *History of Science* (1962– ). In 1964 he became President of the British Society for the History of Science, and from 1968 to 1971 President of the Académie Internationale d'Histoire des Sciences. He had what might be portrayed as a colonial attitude to protocol in continental affairs—although it was not for want of knowledge of what was required. I recall a British protest he led at the IUHPS meeting in Moscow in 1971 at the treatment meted out by the Soviets to a Czech colleague,[12] which had the result that all of us on the national committee had the distinction of being individually photographed by the KGB—an honour that Oxford never equalled.

Crombie was in the last resort neither an organiser nor an orator but a writer, one who influenced large numbers of people in Britain and abroad, most of whom never met him. Many who knew him remarked on his anxiety to appear more English than the English, but this was true only up to a certain point. His origins allowed him to place himself at a due zoological distance from the world as he observed it, so that in Britain he could play the Australian, in England the Celt, in Rome the Englishman, in Oxford the Cambridge man, and so forth. For one who irritated many a colleague by

[12] Josef Smolka, nominated secretary of the International Union of the History and Philosophy of Science, was refused entry, our Soviets hosts claiming that they held a letter from him saying that he did not wish to come. The letter presumably failed to mention his baggage, which was allowed entry, together with the key to his flat.

his way of 'playing hard tennis', as he used to express it, he had an enviably large circle of friends, nationally and internationally. He was a loyal friend to a large number of younger scholars, and if he found it hard to forgive a slight, that was not inconsistent with his resolute defence of basic moral principles. After retirement he took up a half-time appointment as Kennedy Professor at Smith College, Northampton, Massachusetts. He held other visiting professorships in the USA, Paris, Tokyo, and Konstanz, and lectured in many European countries as well as in Australia and India. He was made a Senior Fellow of the British Academy in 1990. Crombie was a member of the Academia Leopoldina (Halle) and of the Pontifical Academy of Sciences—an appointment that gave him great pleasure, for despite his mild anti-clericalism his catholicism meant much to him. He held honorary doctorates of the universities of Durham, Paris X-Nanterre, and Sassari. He received the Forschungspreis of the Alexander von Humboldt Foundation, which took him to Germany for long periods in his last two years; and shortly before his death he was awarded the prestigious *Premio europeo ' Dondi'* (jointly with his old friend Marshall Clagett) for his life's work.

Alistair Crombie took very great pleasure from overseeing the gardens of Trinity, and indeed chief among his recreations at his Boars Hill home was landscape gardening, which he and his wife Nancy (who died in 1993) put into practice there for over forty years. They are survived by four children and their families, now including six grandchildren, and are buried together at Ramsgill in Yorkshire, near the cottage where they spent many of their summers and the church where they were married in 1943.

J. D. NORTH
*Fellow of the Academy*