Chandrasekhar[5] shows that the ratio of the inner to the outer surfaces of the spherical shell, in which convection is occurring, and the nature of the bounding surfaces determine the degree of the spherical harmonics describing the convection which occurs at marginal stability. To account for convection of second degree would require the presence of a core with radius lying between about 0·06 and 0·36 of the lunar radius. These values[6] emerge for the case of a spherical shell with a rigid outer boundary and a free inner one. The low temperature and consequent rigidity of the outer crust of the Moon make the former condition realistic. The latter assumes that the core is of iron, which, as it has a lower melting-point than the silicates surrounding it, may be fluid. It seems reasonable to suppose that the iron has been concentrated towards the centre in the Moon as in the Earth by convection[1]. The closeness of the mean density of the Moon to that of olivine suggests that the proportion of iron is lower than in the Earth, a conclusion not in conflict with the existence of this small iron core (0·2–4·6 per cent of the total volume).

Equating the pressure in the uprising and downgoing convective columns, and assuming a coefficient of volume expansion for olivine of 2×10^{-5} per °C, the 2-km bulge towards the Earth implies differences in temperature driving the convection of about 50° C. Such a theory would explain the discrepancy between the dynamical and the optical determinations of the height of the bulge.

In both the Earth and Moon, convection would bring about an equilibrium between the total heat internally generated and that being conducted out through the surface. As the mantle of the Moon and Earth are of the same composition, probably similar to chondritic meteorites, it is reasonable to suppose that the radioactive heat generated per unit volume is the same. The heat-flow out of the Moon will therefore be one-quarter of that through the Earth's surface, that is, 2×10^{-7} cal/cm^{-2}/sec^{-1}. The heat carried by a convection current is proportional to the velocity and temperature differences. As I have shown[6] the latter in the Earth are only $\frac{1}{3}$° C., the velocity of the currents in the Moon must only be 0·02 of those in the Earth, that is, about 2 cm/yr. As the mean density of the Moon and the Earth's mantle are in the ratio of 3:5 and the gravity of the Moon and Earth in the ratio of 1:6, the buoyancy forces driving the convection in the Moon will be fifteen times greater than in the Earth, for the temperature differences are 150 times as great. Similarly, because the Earth is four times as large as the Moon, the viscous forces resisting convection, if the coefficients of viscosity were the same, would be a third of those in the Earth. Therefore, the viscosity of the Moon must be about 50 times that of the Earth's mantle, presumably the result of the lower pressures and temperatures in the Moon.

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2 Runcorn, S. K. (unpublished results).
3 Baldwin, R. B., *The Face of the Moon* (Univ. Chicago Press, 1949).
4 Jeffreys, H., *The Earth*, fourth ed. (Camb. Univ. Press, 1961).
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OBITUARIES

Sir Ronald Fisher, F.R.S.

Sir Ronald Aylmer Fisher died suddenly at Adelaide on July 29 in his seventy-third year, following an operation. His death is a grievous loss, both to his many friends and to science.

Born on February 17, 1890, the seventh child of G. Fisher, of Robinson and Fisher, auctioneers, of St. James's, he was educated at Harrow and Gonville and Caius College, Cambridge, where he read mathematics, and for a further year held a studentship in physics, studying statistical mechanics, the quantum theory and the theory of errors. He then spent two years as statistician to the Mercantile and General Investment Co., followed by six years teaching in public schools; fortunately for the world, his extremely short sight prevented him from joining the Forces in the First World War.

In 1919 he was selected by Sir John Russell to fill the newly created post of statistician at Rothamsted Experimental Station. Already while at Cambridge he had developed a keen interest in Mendelian theory and its relation to the evolutionary principles of Darwin, and Rothamsted provided a congenial research environment and direct contact with practical biological and agricultural research. This proved a most happy combination, in that he rapidly came to appreciate the real requirements of biological research workers, who have to experiment on highly variable material and are severely limited in the number of experimental units—plants, animals, field plots—that can be included in any one experiment. This demanded, first, that statistical methods should be devised which would enable sound conclusions to be drawn from small samples, and secondly, that the treatments included in an experiment should be so chosen that the maximum amount of information is obtained on the points at issue. On the theoretical side, this quickly led Fisher to the development of exact tests of significance for small samples, and to the development of the theory of estimation, in particular the method of maximum likelihood and the principle of sufficiency. It also led to the recognition of the principles governing experimental design, and to the development of the analysis of variance, which, among its many uses, provides a convenient computational procedure for the estimation of the errors of experimental results and for testing their significance. The modern techniques of experimental design and analysis which flowed from this work have now been almost universally adopted not only in agriculture but also in all fields where experiments on variable material are required. The recent spectacular advances in agricultural production in many parts of the world owe much to their consistent use.

It has been part of Fisher's strength that he always endeavoured to make the statistical methods he evolved available to biologists and others who wish to apply them, and to give adequate insight into the principles without confusing the issue with the mathematics required for their derivation. To this end, after five years work at Rothamsted, he wrote *Statistical Methods for Research Workers*. This book (which has been greatly extended, though little altered, in successive editions) has had an immense influence in introducing modern statistical methods in biological research. *The Design of Experiments*, though not published until 1935, also flowed directly from his work at Rothamsted.

Although at Rothamsted Fisher was not directly concerned with genetics, he pursued his investigations privately, both on the theoretical side and in practical breeding studies on mice, snails and poultry; from the last he confirmed his theory of the evolution of dominance. *The Genetical Theory of Natural Selection*, published in 1930, contains ideas, particularly on human evolution, much in advance of its time. If these have as yet had little practical impact, it may be in part because Fisher's conviction of the power of genetic selection in moulding organisms to fit their environment led him to belittle the importance of reforms, such as improvements in nutrition, designed to improve the environment, and this enabled progressive thinkers to dismiss his theories on human evolution, which in any event they found distasteful, without serious thought. Nevertheless, the book did much to reconcile Darwinian and Mendelian theory.

Fisher's genetical interests led to his appointment in 1933 to the Galton chair of eugenics in the University of London. Here he became interested in blood groups, and in particular resolved the complexities of the rhesus system. In 1943 he became Arthur Balfour professor of genetics in the University of Cambridge, a post which he held until his retirement in 1957. Here he continued his theoretical studies on population genetics and quantitative inheritance, publishing *The Theory of Inbreeding*, and experimental studies on chromosome mapping in mice, and on polyploidy in *Lythrum*. He remained in Cambridge until his successor was appointed in 1959, and then took up work in Adelaide with the Commonwealth Scientific and Industrial Research Organization Division of Mathematical Statistics. After his formal retirement he became interested in the claim that had been made that cigarette smoking was responsible for lung cancer. Whatever the final verdict on this issue, Fisher's re-examination made apparent the weakness of the deductions from the original evidence, and provided an excellent example of the dangers inherent in the statistical analysis of survey material.

Throughout his life Fisher continued to develop his ideas on the fundamental problems of inductive inference and estimation. He early recognized the fallacies of the Baysian approach, and in 1930 introduced the concept of fiducial probability. Other mathematical statisticians have found great difficulty with this concept; but it will, I think, in due course take its place along with likelihood and exact tests of significance of small samples as one of Fisher's outstanding contributions to the theory of statistics. In 1956 he published *Statistical Methods and Scientific Inference*, in which he summarized his current thought on these problems and for the first time put forward the entirely novel concept of reference sets and recognizable sub-sets for defining probability.

Fisher was elected a Fellow of the Royal Society in 1929, receiving the Royal Medal in 1938, the Darwin Medal in 1948 and the Copley Medal, the highest award of the Society, in 1955. He was president of Gonville and Caius College, Cambridge, during 1956–59. He was knighted in 1952, and received recognition from many foreign universities and academies. In 1961 he was elected a member of the Pontifical Academy of the Vatican, an honour which gave him particular pleasure.

To those who knew him well, Fisher was a man of great charm, a brilliant conversationalist, cultured in the widest sense, and appreciative of historical values. He was also a most stimulating scientific colleague, as the many who have worked with him can testify. He did not believe in direction in scientific research—"I wonder how many of us do the work we're paid for"—and this, and his irascible temperament, made him a poor administrator. He was also, I think, unduly sensitive to much of the unjustified criticism which his work, because of its originality, inevitably attracted. This led him into many scientific controversies, which he pursued with vigour and apparent enjoyment, but often in that indignant frame of mind that leads to a partial view of the problem, and leaves unanswered objections that are obvious to the impartial observer. Indeed, his approach to scientific controversy was often that of an advocate rather than a judge, and this did frequent harm to many of the causes he had most at heart.

He married in 1917 Ruth Eileen, daughter of H. Gratten Guinness, and had two sons (the elder was killed in action as a fighter pilot in 1943) and six daughters.

F. Yates

Mr. G. A. Bacon

Mr. Geoffrey Arthur Bacon, whose untimely death occurred on August 1, 1962, was born at Boxmoor, Hertfordshire, on May 25, 1918. He was educated at Berkhamsted School, and in 1938 joined the laboratory staff of the Royal Free Hospital in Gray's Inn Road, where he worked as personal assistant to Prof. R. A. Webb. The unit in which he worked was moved during the Second World War to the Three Counties Emergency Hospital, near Hitchin, Hertfordshire. In 1944 he joined the staff of Harlow Wood Orthopædic Hospital, Mansfield, where he became technician in charge of the Laboratory Emergency Pathological Service. He entered the Scientific Civil Service in 1947 and was appointed for duty as an experimental officer at the then Microbiological Research Department of the Ministry of Supply at Porton. He was promoted to the post of senior experimental officer in 1955. Throughout his service here he has worked entirely with pathogenic bacteria, initially in investigations of the relationship of nutrition to virulence in *Salmonella typhi* and afterwards, over the past twelve years, on the factors determining virulence in *Pasteurella pestis*. Largely through the devoted work of Mr. Bacon these factors, which initially were completely unknown, have now mostly been identified. This increase in our knowledge of the causative organism should permit a more rational approach to the future prophylactic immunization against plague than has been possible in the past. It is ironical, but by no means unprecedented in bacteriology, that one who had devoted such a substantial part of his working life to this end should have fallen victim to the disease he was helping to prevent in others. His death, however, has made us aware that we do not yet have a complete understanding of virulence and infectivity in this organism and that work on these subjects must continue. There is no doubt that Mr. Bacon was fully cognizant of the risks his work entailed, and these risks he willingly accepted.

Mr. Bacon was held in the highest regard by his colleagues. He may best be described as a gentle man who was always generous, kind and considerate and