essential to success, arose from an anatomical consideration. It was known that in addition to their main supply from the pulmonary artery, the lungs also receive a small vascular supply from the systemic circulation by way of the bronchial arteries. These vessels supply the walls of the bronchi and those of the pulmonary arteries together with their associated nerves and ganglia. In preliminary work, Daly traced the territorial distribution of the bronchial vascular system in the lung; he showed that there is a capillary anastomosis between the bronchial and the pulmonary circulations in the region of the respiratory bronchioles, and he was the first to realize the physiological importance of the fact that the arterial and bronchial musculature, with their associated nerves, the very tissues the responses of which were to be the subject of investigation, received their oxygen supply and nourishment exclusively from the bronchial circulation. He therefore devised a method for the separate perfusion of the bronchial circulation as well as the pulmonary circulation in the isolated lung. Previous workers on the isolated lung had perfused only the pulmonary vessels, and their usual failure to obtain vasomotor responses was therefore only to be expected. The technique of the 'isolated perfused lung' had to be perfected before any worthwhile investigations were feasible.

The results obtained have provided convincing evidence of the existence of a nervous vasomotor control of the pulmonary circulation; the earlier evidence had been unsatisfactory. The vasomotor and bronchomotor responses to the stimulation of various nerves and to the injection of various hormones and drugs were fully investigated in several species of animal—guinea pig, cat, dog, pig and monkey. The particular responses obtained varied considerably from species to species and also under varying experimental conditions in any one animal, so much so that no simple or uniform picture emerges. This is characteristic of modern physiology; the more intensively a phenomenon is investigated the more complex is it revealed to be, and the final picture ever recedes from sight. Every day the apparently simple phenomena of classical physiology grow more and more complex as they are subjected to the more searching analysis of modern technique.

With regard to the vasomotor responses, stimulation of either cholinergic (vagus) fibres or adrenergic (sympathetic) fibres most commonly produced vasoconstriction, but in certain defined circumstances vasodilation could result from either. Injection of acetyl choline or adrenaline gave parallel results. The bronchomotor responses were similarly variable, depending on the species and the experimental conditions. The detailed findings are of considerable interest, particularly in their relation to current theories of chemical transmission in the autonomic system, but space does not permit their discussion here.

Perhaps the most significant outcome of this work is to be found not in the detailed findings, but rather in the very variability of the responses obtained; the fact that a given nerve or a given hormone can produce in certain circumstances a constriction and in other circumstances a dilatation in one and the same preparation is in itself of the greatest physiological importance, and suggests many hitherto unthought-of possibilities with regard to the occurrence of similar changes in the whole animal. The problem of the bronchospasm in asthma, for example,

can be considered in quite a new light as a result of these researches. It must always be remembered, however, that the isolated perfused lung is surviving under highly abnormal conditions and probably its behaviour is correspondingly abnormal; so the transference of these results to the whole animal and to man is at present not justified. The potentialities of the lung have been revealed; to what extent they operate in normal life remains to be discovered.

Prof. Narayana concluded his address with a plea for the further development of experimental physiology in India. He himself is already contributing to this end in the active research which he has initiated in his own department at the University of Patna. We can wish our Indian colleagues well in their pursuit of the common ideal, and we shall look forward to their contributions to the further growth of the science.

OBITUARIES

Mr. William Hartree, O.B.E.

A REMARKABLE fact about William Hartree, who died on April 27, 1943, was that his first scientific paper appeared (in 1920) when he was fifty, his last (in 1941) when he was seventy-one. In those twenty-one years he published ten papers alone and thirty-six in collaboration (in *J. Physiol.*, *Proc. Roy. Soc. A* and *B*, *Phil. Trans. A* and *B*, *Phys. Rev.*, *J. Sci. Instruments*, *Proc. Camb. Phil. Soc.*, and *Biochem. J.*).

In the strictest and best sense, Hartree was an amateur; he worked for love of his work and, I think one dare say, of his collaborators. But he worked also with the intensity and pride of a craftsman in his job, and for all his steadfast modesty, he knew, like a craftsman, when he had done it well, and he worked with quite inflexible devotion. During the War of 1914–18, at Whale Island, he said—almost seriously—that he was going on strike for more work and less pay: his pay could not have been much less nor his work more. I had to stop him then from trying to get into the army at forty-seven—because he was enjoying that work too much! Later, at Cambridge, when I pressed him once to take a holiday, he wrote me next day "I have taken a holiday to-day—made a new kind of experiment"—the only kind of holiday he liked.

Hartree was known, except by his work, to very few: he never attended meetings: he wanted no credit or praise for what he did. He almost seemed to prefer to do a difficult job well rather than make it easy. He was ready to face the most laborious calculations in order to obtain a result; he almost resented at first any improvement in technique which made calculation simpler. In his work with me on the physiology of muscle he reckoned that he had written down between 10^7 and 10^8 figures, and we very seldom discovered a mistake. He was always ready to take a sporting chance to see what he could make of a new job—as when he took up research on A.A. gunnery in 1916, when he turned (a mathematician and engineer) to experimental physiological research in 1919, when he took to the theoretical study of atomic structures in 1933 and to the numerical solution of certain partial differential equations, concerned with motion in a viscous fluid, in his last years. Yet in some ways he was very conservative; for thirteen years his chair in the physiological laboratory at Cambridge, where he did his arithmetic,

was precariously balanced on four old fuse-tins of the last war, when it would have been easy to lengthen the legs or even to screw the fuse-tins on! He was not an original or brilliant thinker, though he was obstinately independent when he knew he was right; he preferred always to help others, and this note is written, in gratitude and affection, by one whom he helped full-time for seventeen years.

Hartree was born on April 8, 1870, the son of John Penn Hartree, F.R.C.S. His mother was the daughter of Samuel Smiles, author of "Self Help" (1859) and many other works. J. P. Hartree was the son of an able engineer and, like his son, was educated at Trinity College, Cambridge; he obtained a first-class in the Natural Sciences Tripos (1865). Later he practised, and attended various hospitals in London, but when his son Willie was six years old moved to Belfast. He passed on his temperament and abilities to his son: retiring, silent, able, kindly, with the same high standards and perfection of work. He was a well-known member of the Alpine Club.

From Belfast, W. Hartree went to Tonbridge School and later to Trinity College, Cambridge, where he was eighteenth Wrangler in 1892. Then after a short apprenticeship in electrical engineering he lectured and demonstrated in the Engineering Department at Cambridge until 1913. He married in 1895 his cousin Eva, daughter of Dr. Edwin Rayner of Stockport (sometime treasurer of the British Medical Association) and of J. P. Hartree's sister, and sister of Dr. E. H. Rayner, late of the National Physical Laboratory. Mrs. Hartree has been mayor of Cambridge (his friends always wondered what sort of 'mayoress' Hartree made!) and has done distinguished public work in many fields. Of their five children, only one survives (Prof. D. R. Hartree of the University of Manchester); a younger son who had been a 2nd lieutenant in the Royal Artillery and had worked, like his father and brother, at Whale Island on A.A. gunnery, died soon after the War of 1914-18.

In 1913 Hartree retired and went to live in Surrey, where he busied himself with experimental wireless. Early during the War of 1914-18, he joined up for work with the G.P.O. and served—with his usual devotion and humility—as a telegraph linesman. In 1916, when R. H. Fowler and I were starting up what later became the Anti-Aircraft Experimental Section of the Ministry of Munitions, Hartree, at E. H. Rayner's suggestion, came to join us. It was hard to believe that this shabby, middle-aged linesman, offering to work for love, was an able mathematician and engineer—but he was, and much more. Whenever a job of hard work had to be done on time, whenever some difficult observations were to be made, whenever something was to be fetched or carried, whenever long hours and discomfort had to be endured 'at the far end of the base', Hartree was there. Nobody could see shell-bursts so nearly into the sun, nobody could record what he saw so accurately and quickly, nobody could interpret the results so well, nobody would come so early to the office or stay so late to work them out. Quietly, one day, he improvised a long-base height-finder out of some wires, posts and a steel tape. It came to be called the Hartree height-finder and was used extensively by the troops until sufficient monostatic optical height-finders were produced; perhaps that was why he was given an O.B.E. Once he was arrested on the beach near Great Yarmouth during a gun-trial, for communicating with the enemy by

means of an ordinary field telephone laid along the shore. Once, he was driven to writing verse because the captain of a monitor refused to fire his A.A. gun on a Sunday, the only fine day for three weeks. On Christmas Day 1918 he sent me the four volumes of his grandfather's book "Lives of the Engineers", with a note thanking me for my kindness. Everyone else—by then we numbered forty or so—knew who ought to be thanked.

When it was all over, the arithmetic finished and the instruments packed up, Hartree came to me with a long face and said it was a bad business; he had never enjoyed anything so much, now what could be done about it. Having no intention myself but to return, after nearly five years absence, to physiology, and being young enough to believe that I could (it will be harder this time), I said "Why not come and do physiology at Cambridge?" So Hartree was set to read Bayliss' "Principles of General Physiology", and in a few weeks returned and said that he had not known before that physiology was as interesting as that, and he would come; which he did, for fourteen years. We started off in 1919 on the physiology of muscle, its heat-production, its dynamics, its recovery processes and its various peculiarities; and Hartree learned to make and manage thermopiles, levers, galvanometers, oscillators and Ringer's solution; and—what is much harder for an engineer—to dissect the muscles and nerves of frogs, tortoises and hedgehogs and keep them alive and working. The observations required skill and patience, and the records needed a deal of measurement and calculation—so much the better! Hartree was the first to discover what ought to be called the 'Hartree Effect', that of after-exposure in intensifying photographic records.

I left Cambridge in 1920 for Manchester, moving to London in 1923, but Hartree carried on the work in Cambridge until 1933. On the physiology of muscle and allied subjects in that time came ten papers under Hartree's name alone in various journals, twenty-four under his name and others. We kept in continual touch by post and by as frequent visits as I could pay. I found him always either with his apparatus in a dark room, or perched precariously on his chair on the fuse-tins, doing the arithmetic; never absent, however unexpected the visit. He arrived first in the morning, he usually left last at night—for love.

It would not be fitting, in such joint work, for me to assess its value; but such value as others may attribute to it is due in large part to my collaborator—it certainly would not have been done without him. Some record of part of it is given in "Adventures in Biophysics" (1931).

By 1933 physical disability had made it difficult for Hartree to stand for the long hours necessary in the experimental work, and he felt also that the job which he had set his hand to in 1919 was more or less finished. He turned to his son, D. R. Hartree, for a new one. Fortunately, there were plenty of them that could be tackled at home, and of the kind that needed his patience, accuracy and planned methodical style. The main field of this work was in the calculation of atomic structures, but in the course of time he gave most valuable help in other fields, in particular in some exploratory work on a method for the numerical solution of partial differential equations, in the application of this method to the boundary layer equation in the motion of a viscous fluid, and in connexion with the gas flow in

a tube. I gather from his son that his contributions to the practical calculation of atomic structures have been substantial and important. This work is not routine computing; it is essentially a matter of successive approximations, involving making and adjusting a set of estimates until a set of results, derived from these estimates by calculations which may take a week or two, agree with the estimates themselves; it needs understanding and judgment, far beyond that required for routine arithmetic.

The common experience of father and son at Whale Island, in trajectory and similar work, meant that they could talk the same language of methodical calculation, and another happy and fruitful collaboration developed, lasting until Hartree's death. Their joint papers are to be found particularly in *Proc. Roy. Soc. A*, from 1935 to 1939, while papers by Hartree and M. F. Manning are in *Phys. Rev.* 1940 and 1941. As D. R. Hartree wrote me, "I regard his work for Cu as a considerable technical feat; one aspect of it is that it forms the solution of a system of 34 simultaneous non-linear differential equations!"

This work with D. R. Hartree was done with the same "good companionship" (as E. A. Milne, another of the Portsmouth party, called it) as his previous work with others. "My father," D. R. Hartree writes, "once protested that he did not know the basic theory of the atomic work well enough to be cross-examined on that part of the papers, and so did not think that his name should appear on the title-page." Exactly the same diffidence had appeared in his earlier collaboration with others. "He would have been quite content with a formal acknowledgment at the end, and would not have been particular even about that. But his contribution in these papers was really the main one, without which they would not have been, and it would have been grossly unrepresentative of our relative contribution to the work if it had been published in papers under my name alone." As in physiology, so in atomic physics!

Hartree won a cup for fives at school, his handicap at golf came down to four, he was a good hand at tennis and bridge, and his collection of 'postage paid' postmarks is said to be unique in range and completeness. But of such things he never spoke. For many years before 1916 he had worked, no doubt, as methodically and as carefully as he did later; but there is no printed record of the result. It is strange to think that the method of those ballistic calculations and carefully digested records of observations at Whale Island, and the 'good companionship' of the partnership there, could find so far an extrapolation to physiology and atomic physics at the hands of this shy, gentle, kindly man. The greatest discoveries, however, are made by keeping one's eyes open and one's mind alert for the odd things that turn up—in human relationships as well as science. Hartree turned up in 1916, and those who knew and worked with him since then are richer in both.

A. V. HILL.

WE regret to announce the following deaths:

Mr. W. R. Bousfield, K.C., F.R.S., an authority on patent law, on July 16, aged eighty-nine.

Prof. H. L. Demler, formerly professor of physics in the Technische Hochschule, Dresden, later head of the Department of Physics, University of Istanbul, and finally visiting professor in Rutgers University, on March 22, aged sixty.

Dr. Gerard De Geer, the eminent Swedish geologist, aged eighty-four.

Mrs. E. C. V. Mattick, research worker in dairy chemistry in the National Institute for Research in Dairying, Reading, on July 18.

Prof. E. P. Stibbe, University professor of anatomy in King's College, London, on July 23, aged fifty-nine.

Mr. Fred Stoker, the well-known horticulturist and author on gardening topics, on July 20.

NEWS and VIEWS

Prof. R. A. Fisher, F.R.S.

It has been announced that the chair of genetics in the University of Cambridge is to be filled by the appointment of Prof. R. A. Fisher, who has been Galton professor in the University of London since 1933. Before that time he was head of the Statistics Department at Rothamsted Experimental Station. Prof. Fisher has been actively associated with genetics since 1918 when he showed that the biometrical correlations observed between relatives were to be expected on the Mendelian theory. He has written extensively on the action of selection, and he has shown that the manifestation of any gene must be regarded as modifiable through the action of selection on other genes in the nucleus. This culminated in his well-known theory of the evolution of dominance by selection and in his analysis of the genetical basis of mimicry and natural polymorphism. The mathematical theory of selection in relation to variation and mutation was further developed in "The Genetical Theory of Natural Selection" (1930), which also deals with the genetical aspects of human society. As would be expected, Prof. Fisher has been responsible for many improve-

ments in the statistical analysis of genetical results, especially in the methods of coping with the peculiar difficulties of human data. His early work on the genetical interpretation and statistical analysis of biometrical data has also been extended, notably in a paper published jointly with F. R. Immer and O. Tedin in 1932. This work is perhaps less widely appreciated, largely because geneticists as a body have been shy of attacking the inheritance of quantitative characters; but interest in this subject is reviving, and Prof. Fisher's findings must have their influence on future developments here, as in other fields. It is indeed singularly appropriate that he should both have succeeded Pearson in London and become professor of genetics in the University where Bateson and his associates did so much to extend and amplify Mendel's results, for no one has played a larger part in uniting the once conflicting sciences of biometry and genetics.

University of London: New Principal

MR. HAROLD CLAUGHTON has been appointed principal of the University of London as from August 1, 1943. Mr. Cloughton was educated at