bology Laboratory at Risley had been awarded for four years "because ESRO's financial future after 1975 is uncertain". The initial period of financing was determined by budget release policy and not by any uncertainty as to the availability of funds after 1975.

Last December the ESRO Council unanimously approved not only the Organization's 1972–74 budget but also a substantially increased provisional total level of resources of $110 million a year for the three-year period 1975–77. The Council authorized ESRO to spend not less than $70 million a year on application satellites alone during the period 1974–80.

Yours faithfully,

P. M. BROWN

European Space Research Organization,
114 Avenue Charles-de-Gaulle (92),
Neuilly-sur-Seine

Knuckle Walking

SIR,—The delightful ambiguities that can be produced by simple combinations of English (British/American) words and phrases are never-ending. A case in point is found in the recent correspondence (*Nature*, **236**, 472; 1972) from Mr Tiratsoo referring to the knuckle walking article earlier (*Nature*, **236**, 34; 1972). Mr Tiratsoo, obviously reared in the realm of British football, confuses the authors' expression ". . . football linesmen" . . . as meaning the official who in British-style football prances about (handsomely attired!), waving his little flag and (sometimes?) clutching a stop-watch. What the authors meant, and what must be obvious to an American reader, is the American football lineman—a tackle, guard, or centre who assumes a tripodal stance, cleated feet dug in, one hand in a semi-fist, crouched and ready for the mayhem of scrimmage. In this stance the fingers are knuckled under, pressing into the turf (artificial?) before the snap of the ball from centre. The fingers are knuckled under to avoid the extremely painful injury that can result from having the opposing lineman (275 lb and up) step on outspread fingers. Most American football linemen bandage the hands so that only the knuckle stance is possible. Phrases, like landscapes, are always comprehended with respect to the point-of-view!

Yours faithfully,

C. E. LONG

Nuclear Effects Laboratory,
Kirtland AFB,
New Mexico

Obituary

Professor J. S. Griffith

THE death of Professor John Stanley Griffith at Cambridge on April 23, 1972, at the early age of 43, has brought to an end a remarkably varied career. The fact that his father was a distinguished bacteriologist and his mother a graduate in mathematics and physics may have encouraged him to think widely and discard traditional limitations in his research. In 1948 at Trinity College, Cambridge, he obtained a brilliant first-class in the mathematical tripos. This led to a Rouse Ball studentship, also at Trinity, and the opportunity to take Parts I and II of the biochemistry tripos. His early research was with Sir John Lennard-Jones at Cambridge, and then with Professor C. A. Coulson at Oxford. During this time he was a Senior Demy at Magdalen College. A year of national service at the Admiralty Research Laboratory studying the formation of droplets and the magnetic properties of certain materials was followed by the Stokes Studentship at Cambridge, the Berry-Ramsey Fellowship in Mathematics at King's College, Cambridge, and a series of chairs, first in chemistry at the University of Pennsylvania, then in mathematics at the Manchester College of Science and Technology, and at Bedford College, London. This finally led to a chair of chemistry at Bloomington, Indiana, and—for the last few months of his life—membership of the Institute of Immunology at Basle.

This varied professional career showed itself in his research. His early work was in theoretical chemistry, where he made useful contributions to the newly-developed free-electron model for conjugated molecules, and to the spatial correlation of electrons in atoms. But by 1954 he had begun to turn his attention towards inorganic chemistry. The astonishing upsurge of interest in ligand-field theory which occurred in the late 1950s was in no small measure due to John Griffith and Leslie Orgel. Orgel concentrated on the more pictorial and descriptive aspects, Griffith on the basic principles. His immense book *The Theory of Transition Metal Ions* (1961) was a magnificent piece of work, and for the first time really established the subject on a solid theoretical basis. In it he rationalized the magnetic and optical properties of molecular complexes, taking proper account of temperature effects and spin-orbit interaction. Ten years later it still remains the definitive work in this field. This phase of his life was rounded off by a second book, *The Irreducible Tensor Method for Molecular Symmetry Groups* (1962).

By this time he was getting deeply interested in biological matters. The connecting link was the magnetic property of the combination of oxygen with haemoglobin. How could ligand-field theory account for the newly observed electron-spin resonance spectrum of ferric haem in myoglobin, with peaks at $g=6$? What was the electronic structure of the bond between oxygen and ferrous ion? One fact which had to be explained was that oxyhaemoglobin was diamagnetic, while oxygen itself is paramagnetic. Griffith showed that there were two possible answers. The one, which he preferred, was completely original, and postulated a symmetrical structure, with the iron-oxygen bond perpendicular to the oxygen-oxygen bond. The other was a reinterpretation of Pauling's bent bond structure, translated into molecular-orbital terms. Griffith's interpretation of the electron-spin resonance spectra was immediately accepted, and proved most valuable. His symmetrical model for oxygen binding now seems less likely to be correct, but the question remains open. Recent work on the structure and spectra of haem compounds, including cobalt haemoglobin, suggests that molecular oxygen binds as a superoxide ion, and that many of Griffith's ideas are still relevant.

It was also in the early sixties that he became progressively more interested in the possible contributions that mathematicians could make to studies of the central nervous system. A few of his colleagues tried to dissuade him from moving into this field, but he persisted and was indeed to be followed by some of them. He left Philadelphia and returned to Cambridge to begin his work in earnest. First he studied the statistical properties of spontaneous impulse activity in cortical cells and the way in which activity in a neurone influences the excitability of an adjacent neurone. This work was closely tied to experimental observations.

At the same time he developed a field theory for predicting the overall be-