Unorthodox epidemiology

D.A.J. Tyrrell

Diseases from Space. By Fred Hoyle and Chandra Wickramasinghe. Pp.196. (Dent: London, 1979.) £6.95.

THIS is an unusual book. It is well produced and easy to read, being written in a narrative and popular style; but its six chapters and three appendices are apparently intended to be a serious presentation of a theory put forward by the authors. The subject matter ranges widely. They start with cosmology and their interest in the organic molecules to be found in interstellar space. They suggest that life may have evolved in cometary bodies composed of organic chemicals, which would have had an inner zone protected from radiation and warmed to about 10°C by internal chemical reactions. In this "pool", primitive life-forms like bacteria and viruses evolved and then, when the comet passed near the Earth, were shed into space as micrometeorites, protected from ultraviolet radiation by a coating and sufficiently small not to be overheated on entering the Earth's atmosphere. They then sank towards the Earth, were carried on down to its surface by storms, and thus initiated biological life in the primeval Earth. They further assert that the process continues and that clouds of viruses, particularly those of influenza and the common cold, rain down upon the Earth and cause epidemics of these diseases; and that epidemics of smallpox, and of bacterial diseases like cholera and plague, may occur in the same way.

The authors have obviously read widely and thought deeply, and I am unable to express a useful opinion on their theory of the origin of life. However, as they extend the theory to explain epidemics, this can be examined in relation to present knowledge of infectious diseases. For instance, they argue that influenza viruses descend from space and are breathed in by birds, animals and man, and that the transmission from person to person or animal to animal is unimportant. They do not explain how an organism which must have produced untold billions of particles in a cometary puddle in space (and which they suggest may have arrived on Earth inside a bacterium) was also exquisitely adapted to replicate in avian and mammalian cells. The influenza virus has specific functioning elements by which it penetrates membranes, involves the cell nucleus, is replicated in ribosomes and forms new particles by inserting viral protein into the cell membrane which then bulges out and envelopes the virus nucleic acid and other peptides lying in the cytoplasm beneath. In Earth laboratories many common cold

viruses, rhinoviruses and coronaviruses multiply only in human cells, and only some such cells — in what sort of cells do they replicate in space? The influenza viruses found around 1930 have almost exactly the same internal peptides as those that circulate now — does all influenza come from one cloud of meteorites?

A key fact of their argument is that they have looked at published literature, investigated outbreaks of influenza in schools and found no evidence that influenza spreads from person to person. They state that if influenza did spread in this way it would be distributed in the children in a school by a random process. They show that it is not random — at Eton some houses were heavily affected and others virtually free, as judged by clinical diagnosis — so they prefer the earlier theory that influenza is due to a "miasma". It is stimulating to have current thinking challenged in this way, but I doubt if their theory provides a better model to explain known facts than do our present concepts. There is evidence that different

individuals shed varying amounts of virus, so in small groups they would be expected to infect at different rates — and I think the spread of the recent H1N1 virus from China across the USSR, and to Hong Kong and across the world, is better explained by spread from person to person than by viruses descending from space first in one area and then in the next. I would expect an influenza virus coming from space to appear over a very wide area if it really has to sediment slowly and negotiate the jet streams as the authors describe, even if it does survive desiccation and ultraviolet radiation.

I cannot agree with the theme of the book, which smacks more of science fiction than of science, but I think it is good that it has appeared. I would hate to think that unorthodox views could be suppressed, or that authors could never be published if editors or reviewers did not like their work. □

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US politics and the test ban

Edward Bullard

Blowing on the Wind: The Nuclear Test Ban Debate, 1954-60. By Robert A. Devine. (Clarendon/Oxford University Press: New York and Oxford, 1978.) £7.95.

THIS book is a detailed account of the reactions of US politicians, scientists and the public to the attempts to stop tests of nuclear weapons. It is confined almost entirely to the time when Eisenhower was President (1952-60) and is a political and sociological study rather than a technical discussion. Technical questions are mostly dismissed with the statement that some scientists said one thing and others said the opposite.

To have the events, and the views of the people concerned, set out in chronological order is useful. There are some well-documented judgements that run counter to widely held beliefs; for example John Foster Dulles appears as a strong and consistent supporter of a test-ban.

Useful as it may be, the book makes dull reading. This is, in part, due to the almost exclusive concentration on US views; negotiations require at least two parties and the interest lies largely in the contrast of aims and methods of the two sides. The

extent to which this book ignores all non-US participation is lamentable. The only references in the bibliography to publications by non-American authors are to Khrushchev's memoirs (transcribed and edited in the US), *Sakharov Speaks* (also a US publication) and Harold Macmillan's memoirs. The last of these is the only item in the bibliography that is not published in the US. It is, of course, difficult to understand the motivations and internal struggles of a closed society, such as the USSR; it is, however, important to make the effort. The first route to understanding is the study of official pronouncements and propaganda. If propaganda loses touch with the real world and with real intentions, it loses plausibility and becomes unconvincing to those it is intended to influence or deceive. For example, it was possible to follow the German retreat across Russia in 1944 by noting the westward progression of the locations of alleged German victories. For the Russian views on the test ban there is much more evidence than propaganda.

The 1958 'expert' discussions hold a special place as the first discussions with the Russians in which there was any real meeting of minds. Stalin had been dead for 5 years and Khrushchev had consolidated his power. There was a new situation in which Russian good faith and intentions had to be assessed anew. These matters are inadequately dealt with by the author who seems to have read only the final agreed report and not the transcripts of the day by day discussions, which are much more instructive.

The 1958 meetings were also of interest from another point of view. The American delegation consisted of a mixture of Hawks

and Doves who had not reached agreement on a 'party line' before setting out for Geneva. The reason for this probably lay in Eisenhower's belief in consensus and dislike of controversy and of having to decide between contesting parties. The meetings of the western delegates were often difficult. Most of those present were interested in assessing Russian intentions and motives, but the Hawks were primarily concerned to prevent any kind of agreement with the USSR. They even maintained, for a while, that atmospheric tests could not be sufficiently monitored because the wind might blow round and round in circles and never carry the radioactive debris out of Russia. By confining himself to published sources and Presidential papers the author has missed the details of these interesting controversies. He seems not to have discussed the negotiations with any of the participants.

The breakdown of the negotiations for a complete test-ban came in the political talks late in 1958. The US withdrew its support for the conclusions of the 'experts' concerning underground tests, introduced hastily analysed data from recent tests and declared that the technical matters must be discussed *de novo*. This interesting situation is very inadequately discussed and no references are given to Russian views; for example, to the detailed discussions by Riznichenko.

Throughout the book the discussion of technical matters is marred by an apparent lack of interest in the meaning of numbers. For example, on page 103 it is stated that Willard Libby calculated the amount of Sr-90 in the atmosphere to be 24 megatons. It is doubtful if every reader will understand this oracular statement. Much more confusing are the two values given for the amount of Sr-90 in the soil of the US in 1958 (pages 263 and 272); these differ by a factor of a million. The larger of them is a factor of 1000 less than that given by the AEC for 1961 in *Effects of Nuclear Weapons*. This muddle may be connected with the author's belief that m is the abbreviation for micro.

Perhaps I have been unduly severe, the book is about American politics and my complaints are that it is not about other aspects of the subject. It does unearth some fascinating things, such as a remark made in 1958 by a member of the President's Scientific Advisory Committee. He (who was he?) is said to have said: "These men who don't want a test moratorium are like a kid you are trying to put to bed. First he wants a drink of water and then he wants to go to the bathroom, but what he really wants is not to go to bed". □

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Commitment to logic

Aaron Novick

Origins of Molecular Biology: A Tribute to Jacques Monod. Edited By A. Lwoff and A. Ullmann. Pp.246. (Academic: New York and London, 1979). \$17.50; £11.35.

MOLECULAR BIOLOGY had its origins in several centres which served as gathering places for people from around the world. In the very beginning, it was Cold Spring Harbor in New York and soon after the Institut Pasteur in Paris and the MRC Laboratory in Cambridge, among others. These places developed for a variety of reasons not the least important of which was the presence of some of the leading figures in this new field. At the Institut Pasteur it was André Lwoff, Jacques Monod and Francois Jacob. Jacques Monod died in 1976 and a number of his colleagues and disciples have contributed to this *Tribute*, a volume which describes from a number of points of view an exceptional person who played one of the major roles in the development of our present understanding of the control of gene expression. From its title, it seems to be a companion volume to *Phage and the Origins of Molecular Biology*, edited by J. Cairns, G.S. Stent and J.D. Watson (Cold Spring Harbour Laboratory, 1966), a volume dedicated to Max Delbruck on his sixtieth birthday.

The personal accounts in volumes such as these provide descriptions of important and fascinating details in the development of scientific ideas never reported in the usual forms of publication. Because of its memorial character, the present volume includes many personal details in its descriptions of a man of great spirit whose life outside the lab as well as in it was of heroic scale, whose friends included scientists and public figures throughout the world.

Of particular interest is the collection of personal statements from most of the people involved in the series of discoveries which culminated in the giant step represented by the Jacob-Monod model for gene expression and regulation. The reports provide a fine illustration of the development of a great scientific idea, but one must recognise that the picture is limited by the modesty of some of the participants in this memorial volume. André Lwoff and Francois Jacob were no less important both to the exciting and creative spirit at the Pasteur and for their own remarkable scientific contributions. As a history, the volume does not make it adequately clear that the critical experiments which showed that control of the *Lac* gene occurs through action of an endogenous repressor could be done only

because of Jacob's timely development of the necessary genetic technology.

It is also fascinating to follow the development of Monod as a scientist. Here is a man for whom science was a great personal adventure and a means of personal expression, yet a man fervently dedicated to logic and reason. He exulted in the power of deduction and was known to shout after an observation verified one of his deductions that this was again proof that Descartes was right and Kant wrong. Despite his commitment to logic, he had a remarkably successful intuition for how things must be, and he could present these pictures with great force. Even when he was later proved wrong, the vehemence and sharpness of his argument expedited the clarification of a problem.

Some of the authors also note his audacity. He never limited himself to the disciplines of his education nor did he ever restrict himself to minor objectives. In his crowning work, the development of the allosteric model for regulatory proteins, despite being trained as a zoologist he used modern physical chemical ideas with no inhibitions to formulate a remarkably original and innovative theory. "It is no longer a question of right or wrong", one of my colleagues in the field remarked. "The Monod theory has changed the style of thinking among the biochemists."

An impressive feature of the volume is the honesty of some of the reports which describe the personal faults as well as the greatness of Monod. His enthusiasm and pursuit of fundamental problems inspired the people he worked with. His candour came as an exciting surprise when it was first encountered by his disciples. This lack of stuffiness along with his generosity and the confidence he placed in his students created love and great loyalty. But he also had personal faults, and these too are reported. He could be vain and arrogant in a pointless way. Surely it is because they held him in such high regard that his colleagues were free to describe these faults so honestly.

As occasional allusions in the volume indicate, Monod's life outside of science was also remarkable. He was aware, wise and, when he felt it necessary, active in the world of politics. During the hysteria in the United States in the early 1950s, when foreign visitors were required to sign non-communist affidavits, the *Bulletin of the Atomic Scientists* published statements by a number of European scientists; I recall that by far the best of these was the one by Monod, who with characteristic perception saw the larger implications of US policy. Again, on the occasion of the Nobel Prize, rather than basking in the adoration and pride the French have for their Nobel Prize winners, he took advantage of the public attention to improve the support of science in France by making a strong public statement severely critical of his government's science policy,