Riding roughshod over cancer research

John Cairns

The Biology of Cancer: A New Approach. By P. R. J. Burch. Pp. 452. (MTP Lancaster, January 1976.) £11.50, $23.23.

Professor Burch, Photo: Leeds University

PROFESSOR BURCH has recently acquired a certain fame thanks to his much publicised assertion that cigarette smoking is not a cause of lung cancer. This is just one consequence of his theory that most diseases are largely determined by random events within us that owe nothing to external agents in our environment. He has now written a detailed account (one might call it a polemical tract) describing how he came to his present, unorthodox position.

The argument runs as follows: Over most of our lifespan the incidence of the common cancers rises roughly as the fifth power of age. This is usually explained by saying that cancer is the end result of several independent steps (mutations) whose frequency among the cells at risk accumulates linearly with time. On further scrutiny it turns out, however, that many other diseases—ranging from arteriosclerosis and osteoarthritis to certain forms of psychosis—show a similar relationship to age. As these diseases are clearly multifocal in origin, they can hardly be ascribed to a particular set of rare mutations having occurred in one of the cells at risk in artery, joint, brain, and so on. If we want to persist with multistep models for disease, we should therefore try to come up with some formulation that allows a set of mutations in one cell to be the cause of unifocal or multifocal changes among other cells. So let us postulate that the main tissues of the body are divided up into a large number of subsets containing a few hundred cells whose growth is controlled by a central system of cells divided into a similar number of subsets, and let us postulate that most diseases are due to defects in the central controlling cells; unifocal diseases like cancer are due to mutations that fall within a single subset of controlling cells; multifocal diseases like arteriosclerosis are due to mutations in the primordial stem cells which are the parents for many subsets of controlling cells. Burch goes on to suggest that the central controlling cells are lymphocytes, that the dialogue between central and peripheral systems is conducted by means of pairs of specifically interacting molecules, and that these molecules are coded for by genes which contain palindromic base sequences and therefore are apt to be read backwards as well as forwards.

The theory owes an acknowledged debt to such notions as the control of growth by 'chalones', the recognition of self and not-self, the creation of 'forbidden clones' and the control of cancer by immune surveillance—ideas championed by many people, Burnet in particular, at one time or another. As formulated by Burch the theory provides a plethora of independent variables—the mutation rates for the particular genes involved, the number of genes that must be altered to inactivate an appropriate central controlling cell, the number of such cells that have to be affected, the proportion of people that are genetically susceptible, and so on. Because some of the functions that can be generated with the model do not increase steadily with time but go through a maximum, the theory can be made to take in even such things as the age distribution of a childhood disease like measles (for me, the clinching *reductio ad absurdum*). Further, as Burch modestly points out, "so far as I am aware, our theory is the only one that offers a general solution to the problem of the anatomical specificity of the lesions of natural disease".

Now, all this curve fitting would be just innocent fun, albeit of a rather old-fashioned kind, were it not that somehow the idea of chance events scattered randomly among central controlling elements has been taken to mean that the events cannot be due to external agents. I must say I do not understand the logic here; after all, one of the first collections of biological data shown to follow the Poisson distribution was the number of men killed by horses each year in the Prussian army, and there the external agent was plain for all to see, on the hoof as it were. Anyway, that is what Burch believes and so he sets out to show that the incidence of the common cancers is not perceptibly influenced by environment and therefore that nothing can be done to prevent cancer, or most other diseases if it comes to that. (I might sympathise with this attitude if he were an old scientist subconsciously wishing to make the virtues and rewards of science decline to zero coincidentally with his retirement, but Burch is hale and hearty as far as I know and has no reason to go round preaching a kind of molecular nihilism).

Obviously the theory rides roughshod over most forms of cancer research. For example, the connection between external agent and susceptible cell is too direct to be denied for the various experimental cancers in animals and for most industrial cancers, and so Burch is forced to say that these are exceptional and have no bearing on the common 'natural' cancers. As for the copious epidemiological evidence that the incidence of the common cancers changes with alterations in habits and environment, he argues for a combination of illusion and conspiracy—for example, Japanese migrants to the US show altered cancer rates because they have a genetic constitution that drives them to migrate to a country where coincidentally the population suffers the same spectrum of cancers as they themselves; similarly, the apparent steady decline in stomach cancer and rise in lung cancer over the past 50 years are artefacts due to changes in diagnostic fashions.

Now, I believe that all this is what might euphemistically be called wrong. Indeed, I think that even Burch will come round to that opinion. Were the biology of cancer a normal branch of science, his unorthodox beliefs might be stimulating and would add a certain piquancy to the lives of cancer research workers, like the pleasure one gets on meeting someone who really believes the earth is flat. Unfortunately the biology of cancer is not that kind of subject. This book will undoubtedly persuade many people to continue smoking who might otherwise have stopped, and may confuse those who legislate on the search for and control of carcinogens in our environment. Some of these people will, I am sure, have been persuaded not by actually reading the book and being swayed by the inexorable logic of his argument, but simply by the thought that when a professor writes a fat, obscurely mathematical tome on any subject whatsoever, there must be something in what he is saying. And that is why I wish this book had not been written. □