officials who are to use it, or more data bits than anyone can count in a year, the chances of failure are very high. Fourth, the rule of thumb: if the data are thicker than your thumb (skeptics—see rule 1—may say "pinky") they are not likely to be comprehensible to anyone. The fifth rule is to be like a child. Ask many questions; be literal in appraising answers. Unless you understand precisely who will use each data bit, how often, at what cost, relevant to which decisions they are empowered to make, don't proceed. Sixth is the rule of length and width, or how to determine whether you will be all right in the end by visualizing the sequence of steps in the beginning and middle. Potential users of information should be able to envisage the length of the data flow over time, that is, who will pass what on to whom. If there are more than three or four links in the chain it is likely to become attenuated; data will be lost, diverted, or misinterpreted. The width of the chain is also important. If the data go to more than one level in the organization, the chances that they will be equally appropriate for all are exceedingly slim. The longer the sequence of steps, the wider the band of clientele, the less likely the information is to be of use. Seventh, the rule of anticipated anguish (sometimes known as Murphy's Law): most of the things that can go wrong will. Prepare for the worst. If you do not have substantial reserves of money, men, and time to help repair breakdowns, do not start. Eighth, the rule of the known evil. People are used to working with and getting around what they have, they can estimate the "fudge factor" in it, they know whom to trust and what to ignore. They will have to reestimate all these relationships under a new information system, without reasonable assurance they will know more at the end than they did at the beginning.

Ninth comes the most subtle rule of all, the rule of the mounting mirage. Everybody could use better information. No one is doing as well as he could do if only he knew better. The possible benefits of better information, therefore, are readily apparent in the present. The costs lie in the future. But because the costs arrive before the benefits, the mirage mounts, as it were, to encompass an even finer future that will compensate for the increasingly miserable present. Once this relationship is understood, however, it becomes possible to discount the difficulties by stating the tenth and final rule: Hypothetical benefits should outweigh estimated costs by at least ten to one before everyone concerned starts seeing things.

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Virology and Cancer

The Molecular Biology of Tumor Viruses. JOHN TOOZE, Ed. Cold Spring Harbor Laboratory, Cold Spring Harbor, N.Y., 1973. xxii, 744 pp., illus. $16. Cold Spring Harbor Monograph Series.

Peyton Rous once told me that when in 1911 he discovered that a chicken sarcoma was caused by a virus (a discovery for which he received a Nobel Prize in 1966) an eminent pathologist stated that Rous sarcoma could not be a cancer since Rous had discovered its cause—and it was well known that the cause of cancers was unknown. That kind of reasoning did prevail a long time in cancer research. Slowly, however, there emerged a large body of regularities which forced even the metaphysicians to admit that cancers, like all physical phenomena, have causes and that some cancers are caused by viruses. More recently, the balance has swung, and a substantial body of oncologists are betting on viruses as *the* cause of all cancers—not, of course, by the rather trivial path of infection, as for measles, polio, or the common cold, but through more subtle relations between the cellular genes and viral genes obscurely hidden within the cells.

The present book, therefore, comes at a most appropriate time. It provides, in a detailed if not always delectable form, the essential background both on cells as responders to tumor viruses and on the viruses that have been incriminated as causes of cancer. It presents the various current theories and their justifications in an impartial although not detached way.

This book is clearly not meant for readers interested in an overview of the field. Its 13 chapters are decorated with sets of references ranging from 100 to 400 a chapter. Written mostly in 1969 and 1970 on the basis of two tumor virus workshops held at the Cold Spring Harbor biological laboratory, it has been brought up to date to late 1972 and occasionally even later under the editorship of John Tooze. Twenty-two contributors are listed, interestingly, without attribution of specific sections, evidently because the rewriting was done by one or two people. This procedure is validated, in the opinion of this reviewer, by a homogeneity and excellence of style such as could hardly have been expected from 22 scientists.

Yet, the book, as stated above, is not a book for the biomedical public in general but for specialists, more specifically, for the young scientists ready to enter the exciting field of tumor virology as well as for all cancer workers wanting to be up to date in this forefront area of cancer research.

In the tradition of previous Cold Spring Harbor monographs—*The Bacteriophage Lambda* and *The Lactose Operon*—the present book is evidently meant to be useful. Much more than the two other monographs, it is effectively organized for use and study. It opens with a historical survey, already dense with current ideas, followed by two chapters on mammalian cells in culture and on cellular surfaces. Then it deals with the DNA-containing tumor viruses, adeno-, herpes, and "papova" viruses (this reviewer seems to be the last virologist left who refuses to use silly acronyms as names of viral groups), thoroughly exploring the phenomenon of cellular "transformation" to a malignancy-like form.

The last four chapters, on RNA tumor viruses, are of course the most intriguing, since it is viruses of this group (which includes Rous's original isolate) that are looked upon by some virologists as possible causes of human cancers as they are of cancers of fowl, mice, and other mammals.

The reader should be aware of a major source of the excitement that lies underneath the dry surface. The tumor viruses have not much RNA or DNA—maybe 5, maybe 10 or 15 genes. Any one of these genes may be "it": the gene that makes for cancer. The excitement of the tumor virus workers—the sense of zeroing in on one of the greatest and nastiest secrets of nature—projects itself only occasionally out of this book's factual presentation of the experimental landscape.

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