tobacco carcinogenesis as of the end of 1965 and should prove useful to those interested in the physical, chemical, and biological properties of tobacco smoke. The book is well printed and bound, although some of the photomicrographs are not of the highest quality.

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Essays by Kapitza

Collected Papers of P. L. Kapitza. Vol. 3. D. TER HAAR, Ed. Pergamon, New York, 1967. x + 244 pp., illus. $13.50.

This volume of Kapitza's papers is devoted to general, semipopular surveys of technical subjects, biographical essays, and essays on the organization and future of science and its impact on the human condition. Its primary interest, therefore, lies not in its scientific content in the narrow sense but in the insight it provides into the thoughts and the development of ideas in the mind of one of the great physicists in our time—one who has not only scaled the Olympian heights of pure research but has battled the tides and achieved safe passage through turbulent and treacherous crosscurrents of political pressure, retaining loyalty to principle and conscience.

Imperfect analogies are easily constructed. We have a strong natural urge to seek them out and embellish them when comparing lives of great scientists whose names and whose works contribute to the substance of our learnings as well as teachings. This reviewer found in the pages of these writings parallels between the careers of Peter Kapitza and J. Robert Oppenheimer. Both were developers of leading schools of physics in their respective nations, to which they returned (albeit in different circumstances) from European centers of learning. Both were distinguished organizers in fostering the growth of modern physics in their countries. Recognizing the importance of big facilities in the future of experimental science, both were closely associated with the development of "big physics" in their respective countries. Both were driving forces during the Second World War in their countries' wartime science efforts. Both suffered precipitous falls from grace when they refused to give way to hysterias of the political moment that were running rampant in their countries; and both were eventually vindicated and enjoyed rehabilitation as heroes. As articulate, cultured scientists and statesmen of science, both have deeply and eloquently expressed their concerns for humanity's survival in a nuclear age.

The papers in this book span more than half a century, from the teenager's investigation of the production of cod liver oil that appeared in 1913 to recollections of Lord Rutherford first published in the 1966 *Proceedings of the Royal Society* on the occasion of Kapitza's return to the scene of his earliest scientific fame. To this reviewer, the perceptive and beautifully written essays on the lives of great scientists—Rutherford, Newton, Benjamin Franklin, Paul Langevin, Lomonosov, and Niels Bohr—comprise the most enjoyable portion of this volume. Not only is the style charming, but the color is rich, and the scientific examples and anecdotes that spice these biographies are both entertaining and substantive in transforming them into significant essays on the essence of science. They should be valued in the teaching of young scientists to help convey the excitement and the fascination of natural science.

It is also interesting to follow through the line of Kapitza's thinking in the 1930's and 1940's that led him to concentrate his research on producing strong pulsed magnetic fields for studying important dynamical properties of atoms and on producing liquid air in quantity in order to pursue the low-temperature research that led to deep insights into the property of superfluidity in liquid helium II. He always strove to extend the realm of physical parameters amenable to measurements, quoting Davy's "One good experiment is worth more than the ingenuity of a brain like Newton's." (Speaking as a present-day particle theorist hoping to see new accelerators bring still higher energy events into the realm accessible to quantitative study, I fully concur—especially as I labor in the current theoretical morass and cacophony of concepts.)

Some of the essays of the 1940's are political period pieces, but it is timely to reflect on Kapitza's dialogue with Bertrand Russell in 1956 in which he remarks:

> . . . in concluding international agreements which have the aim of preventing atomic war, it is necessary to take into account the possibility of inventing an effective means of defence. In such agreements it is necessary to provide for mutual notification of the course of experimental work on defensive measures, though this may complicate the conditions of mutual supervision over fulfilment of the agreement.

Updating this observation, on his recent visit to Britain in 1966, Kapitza was quoted in the New York *Times* of 5 May 1966 as having suggested "as his formula for peace an international 'exchange of scientists from military institutions.' 'Then there would be no more secrets,' he observed with a puckish smile, adding that he supposed it would not come to pass 'for some time, you know.'" As the world's strategic arms race continues with new and better ICBM's, joined now by ABM's, can someone come forth with a better idea?

The much honored career of Kapitza needs no recapitulation or synopsis in this review. His name is too well known and respected by the scientific community. His book should be enjoyed for what it does, namely give us a charming and at the same time significant glimpse into the thinking and ideas of an important scientist of our times who has the ability of expressing his thoughts clearly and of writing in a delightful manner, whether discussing the future of science in society, reminiscing about the great men of science, or describing how he himself was drawn to his own important scientific work.

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Assaying Chromosome Damage

Human Radiation Cytogenetics. Proceedings of an international symposium, Edinburgh, Oct. 1966. H. J. EVANS, W. M. COURT BROWN, and A. S. MCLEAN, Eds. North-Holland, Amsterdam; Interscience (Wiley), New York, 1967. viii + 218 pp., illus. $11.50.

One consequence of the development of an easy and reliable technique for culturing human somatic cells has been a rash of reports dealing with the induction, by ionizing radiations, of chromosomal aberrations in peripheral lymphocytes. These reports have dealt not only with studies in vitro but also with studies in vivo stemming from accidental and therapeutic exposures. Understandably, the large and diverse group of researchers utilizing their own modifications of a basic technique have

published, not too infrequently, varying estimates of the degree of damage to human chromosomes produced by ionizing radiation. Consequently, in recent years a controversy has developed over how reliable the data from differing laboratories might be.

This conference was thus originated to bring the workers in the field together to discuss, informally, the ramifications of the peripheral-lymphocyte culture technique and what effect they might have on the quantitative data. Such a conference was desirable, but it is perhaps unfortunate that its culmination should be a formal publication of some of the topics under discussion—unfortunate in that, as a cursory glance at the list of invited participants reveals, there are glaring omissions of contributors to the field, as well as absentees among those invited, and the publication, without scientific review, includes some outdated and inadequate papers.

Perhaps the absence of some of the invited participants can be attributed to the timing of the conference. It was held three months after the International Congress of Radiation Research at Cortina d'Ampezzo and would have necessitated two trips abroad within a two-to-four-month period for many individuals. It would have been better to hold the conference as a satellite meeting of the Cortina meetings, as that would have enabled many more interested workers to participate.

With respect to the published contributions, it becomes quite obvious that the major disagreement on the assessment of the chromosomal aberrations is between the British and American workers. Evans pointed out that the coefficients of aberration production reported by the "American workers" are lower than those found by himself and his colleagues. He argues that this is most likely due to the Americans' having selected 72 hours, a time at which the cultures are purported to consist predominantly of cells at the second post-irradiation mitosis, as a time to collect metaphase figures. Evans himself has selected 50 to 54 hours, a time when, he argues, the cells are at first post-irradiation mitosis. An interesting point is that Norman reports, in the same conference, that the coefficients he obtains at a 50-hour sampling time are the same as those found by Bender and his colleagues at 72 hours and are significantly lower than those reported by Evans. It appears there are more complications that affect observed aberration yields than merely the point in the life of the culture when samples are collected.

If I had been asked by a journal to review these articles for publication I would have advised not publishing a large number of them. One paper, for example, discusses the effect of sampling time in the history of culture on the yield of aberrations. A total of 19 sampling times at two doses were examined. At seven of these sampling times 25 or fewer cells were analyzed, and at one point there was only one cell analyzed (a standard error is also given). On the other hand, there were quite interesting presentations concerning relatively new areas of study and recent insights into old problems. Nowell's short paper dealing with the immunological memory of circulating lymphocytes was found to be very interesting, as was the work of Buckton *et al.* on estimating dose-response relationships following irradiation in vivo. These are areas of obvious importance in our understanding of and ability to evaluate the data obtained from laboratory-designed and accidental exposures of human cells to ionizing radiations.

The published proceedings make one point very clear: there are many unknown factors involved in the initially apparently simple peripheral-lymphocyte culture system that make interpretation of the data difficult. Furthermore, it is evident that the time has arrived for a concerted effort by all groups to have an "informal" meeting to attempt to glean some value from an obviously important field.

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Photoreactions

Preparative Organic Photochemistry. ALEXANDER SCHÖNBERG, in cooperation with Günther Otto Schenck and Otto-Albrecht Neumüller. Second edition. Springer-Verlag, New York, 1968. xxiv + 608 pp., illus. $37.

This volume appears ten years after the first edition (which was written in German). The sizable expansion of material in the second edition (which is in English) reflects the explosion of photochemical papers that has occurred since 1958. The new edition consists of 45 chapters of photoreactions classified by reaction type. A 46th chapter, by G. O. Schenck, reviews light sources and filters for use in preparative organic photochemistry. A comprehensive bibliography of previous published reviews of photochemistry is also included. Finally, there is a thorough index of the book's contents according to author, reaction, sensitizer, and compound. The literature is covered up to the end of 1965.

The guiding principle of this volume is to provide a detailed survey of preparative photoreactions for the organic chemist. Only preparative aspects of organic photochemistry are included, so that other monographs must be consulted for theory.

The author has provided chemists with a splendid, comprehensive source of organic photoreactions. Many preparations are described in detail as examples of various reactions. The size of the book, unfortunately, demands a high price. Nevertheless, every worker in the field will want this important reference source in his library.

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Trees

Gehölzphysiologie. HORST LYR, HANS POLSTER, HANS-JOACHIM FIEDLER, *et al.* Fischer, Jena, 1967 (distributed in the U.S. by Abel, Portland, Ore.). 444 pp., illus. $12.90.

There are very few textbooks of tree physiology. The last major work was that of Büsgen and Münch, which appeared in German in the late 1920's and was also made available in English. It is very unlikely that Büsgen and Münch's book will be replaced as a whole, because it contains a great deal of descriptive information on growth habits and growth form which is still valid. Modern writers can therefore concentrate on those topics about which our knowledge has changed and substantially increased. To write a physiology text about a specific group of plants has its problems. First of all, the question arises of to whom the book is directed. Will the readers be mostly students who take it as their only plant physiology text, or will basic plant physiology be a prerequisite? In other words, how broad or how specialized should a tree physiology text be?