certain fields. Nobody, I hope, wants a federal prescription of research undertakings. I can only say that if investigators and institutions are uninterested in gerontology, there is little we can do. NIH can only go as far as Congress and the scientific community permit" (*Geriatrics*, Sept. 1965, p. 77A). The statement stands in marked contrast to the recommendations made by the scientific community at the White House Conference and are a source of discouragement to those who are deeply committed to work in this challenging field. This reviewer would suggest that the following program could bring some semblance of order into the present disorder:

1) The appointment of a 10- to 15-man biological research advisory committee to outline the various promising avenues for basic studies of the mechanisms of aging. It can readily be demonstrated that the number of scientists who are qualified to serve on such a committee by virtue of interest in the problem as well as by professional standing is more than adequate.

2) Establishment of a study section within the NIH (or the NSF) to stimulate effective attack on the topics thus outlined, thereby encouraging relevant grant applications and the growth of appropriate training programs.

3) Expansion of the human physiology and psychology program of the Gerontology Research Center in Baltimore and transfer of its basic-biology component to a suitable academic environment.

4) Creation of a National Institute for Aging Research, either in NIH or in another suitable governmental agency.

If these or analogous steps are undertaken promptly, one may expect that within the next decade there will be the kind of progress that Szilard knew to be possible in the last decade; and in 1977 a book review such as this would be an undiluted discussion of solid achievements.

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Notes

1. In 1955, grants for research on aging totaled less than $1 million (*U.S. Public Health Serv. Publ.* No. 799, p. iii); in 1961, the total—for 245 grants—was $5.7 million, or, if studies secondarily related to aging are included, $16.2 million (*U.S. Public Health Serv. Publ.* No. 841, p. iii); in 1966, there were 64 grants totaling $4.6 million (NICHHD, *Program Statistics Branch Rept.*, 26 Oct. 1966, and *U.S. Senate Report, 90th Congr.*, No. 169, p. 69, 12 April 1967).

Augmentor of the Human Eye

The Evolution of the Microscope. S. BRADBURY. Pergamon, New York, 1967. 367 pp., illus. $12.50.

This book contains a fascinating, well-illustrated, and very readable description of the development of the microscope from its first beginnings around 1610. Soon after the Galilean and Keplerian telescopes had been invented it was observed that they could be converted into microscopes by moving the eyepiece far enough from the objective to permit focusing on a near object. However, the poor definition of early compound microscopes led many experimenters, including Leeuwenhoek, to prefer homemade simple magnifiers, which were often of extremely high power.

The historian of the microscope is fortunate that the early developments were well documented. Numerous diagrams and engravings of early equipment are reproduced in this book, and many photographs of early microscopes, one dating back to 1678, are shown.

The essential parts of a microscope system, namely, a light source, condenser, object holder, objective, and eyepiece, with coarse and fine focusing adjustment, were understood and embodied in microscopes described by Hooke and others as early as 1665. Great improvements were made during the next hundred years, and indeed some microscopes made as early as 1780 bear a strong resemblance to present-day instruments in their general external appearance. The microscope, unique among scientific instruments, early became a thing of beauty, and at one time was considered a suitable plaything for a king.

Bradbury has not overlooked the development of the optical parts of the microscope. Achromatic objective lenses were made by Chevalier and others as early as 1808, although these were designed by empirical methods. The theory of the objective was firmly established by the work of J. J. Lister in 1830. Water-immersion objectives for biological studies date from 1867, and Abbe developed his "homogeneous immersion" principle in 1878. Much praise is given to the designers who brought microscope optics almost to their present state of perfection around the end of the last century.

This book is packed with interesting facts about early microscopes. The author obviously loves his subject and thoroughly understands the purpose of everything he describes. His numerous verbatim quotations from early writers are interesting and serve to illuminate the story he tells. The author appears to have had the good fortune of being able to examine many of the ancient instruments which are still in existence in European museums, so that his descriptions are first-hand and critical.

The electron microscope is dealt with fully, and other modern developments such as ultraviolet, phase, and interference microscopy are briefly discussed. There is no mention of zoom, metallographic, or petrographic microscopes, or of flat-field microscope objectives. I detected no obvious errors other than the spelling of some proper names, Zernike and Greenough in particular. The brief index is scarcely adequate for the mass of valuable information contained in this excellent book.

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On Chemical Kinetics

Gas Phase Reaction Rate Theory. HAROLD S. JOHNSTON. Ronald, New York, 1966. 372 pp., illus. $10.

This well-written monograph is a useful addition to a field with a seeming surfeit of texts. As the title suggests, its scope is too restricted to make it a suitable substitute for Frost and Pearson or Laidler as the required text in the typical senior-first-year-graduate course in chemical kinetics, although it does have short, introductory chapters on quantum mechanics, potential functions, and statistical mechanics that students would find interesting. What this book sets out to do, and does do very effectively, is to highlight the similarities in inexactness and incompleteness between applied "collision theory" and "absolute rate theory." It then builds a case for espousing neither point of view to the exclusion of the other. In reality few practitioners of either theory are so partisan that they do not adapt to their own noble designs features of the "opposing" theory that they find especially suitable, but Johnston's slightly contentious thumping for a superposition of the two theories is useful in stirring the blood of the usually torpid reader. For instance, on page 323 Johnston points out that for bimolecular reactions with activation energy one can consider three sets of