In tracing the historical development of knowledge of cancer, the tremendous role played by French workers becomes evident. The scientist most often quoted is Borrel, who was among the first to suggest that cancer, including that of the breast, was caused by virus. He adhered to these ideas despite insufficient and often contradictory evidence. No wonder that in a leading review of Oberling's original "Le Problème du Cancer" Borrel is cited in five paragraphs and Peyton Rous is not mentioned, though it was the latter who, with associates, during several decades of systematic research, accumulated a wealth of facts on which the virus theory of cancer rests. These facts will last even if the virus hypothesis should fall. They are duly recognized and related by Oberling, and many of the convincing arguments presented by him in favor of the virus theory are those of Rous and associates.

"The Riddle of Cancer" is as informative as it is interesting. Research workers in the field of cancer will often find their opinion questioned or contradicted; nevertheless, they will be stimulated by this book. The correctness of some of the entertaining stories "behind the curtain" is difficult to ascertain. Hanau, the first to transmit a tumor, is stated to have found his work shamefully neglected and in umbrage destroyed himself. Hanau had carcinoma of the large intestine, which recurred after two operations[1]; this does in part explain his suicide at the age of 41. Eloquence and wit are often coupled with overstatements, *e.g.*, that transplantable tumors are incapable of furnishing information applicable to man. From the time of the brilliant paper of Hanau up to the current papers of Greene on heterotransplantation of tumors, experimentation with transplantable tumors has contributed much to the understanding of the phenomena of cancer. Since tumors arising and transplanted in the homozygous stocks behave almost like the animals' own tumors, they may be found useful in types of experiments where heterozygous mice can not be employed. Investigators must, of course, be familiar with the limitations and pitfalls of working with transplantable tumors.

The hypotheses concerning the immediate cause of cancer are well reviewed, and the conclusion is reached by Oberling that only the virus hypothesis is compatible with all known observations. According to him, the great virtue of the mutation hypothesis is that it leaves the way open for any etiological conception; its great fault, that it explains nothing. Is it not a good explanation of autonomy, the most important feature of a neoplastic cell? Among the other objections to the somatic mutation theory of cancer are that somatic mutations are rare in mammals and the few that are known invariably effect secondary changes like pigmentation; they are never associated with such a radical change as malignant transformation of cells. These are hardly convincing arguments. The strongest point against the somatic mutation hypothesis is the observation that often the development of cancer appears to be a step-by-step change. In the opinion of Oberling, "Alone among the various hypotheses, that of the viruses seems to have a real chance of leading to the goal." Observations in support of the virus hypothesis are well reviewed, whereas those against its acceptance are minimized. Among these is the individuality of each cancer, which in terms of the virus theory would require a distinct virus for each tumor in each individual.

The French edition appeared in 1942. The Jane Coffin Childs Memorial Fund made this work available to the English-speaking world by entrusting its translation, nay, its recast, to no less a craftsman than William H. Woglom. The reader may miss the charm of the French original, but Woglom has accomplished what is seldom done, the pouring from one golden goblet into no baser a metal.

JACOB FURTH

[1] "Ein bewegtes Gelehrtenleben." By Otto Lubarsch, Berlin, 1931.

PERUVIAN ARCHEOLOGY

Peruvian Archeology in 1942. By A. L. KROBER. Viking Fund Publications in Anthropology, Number Four. 151 pp. 48 plates. 8 text figures. New York: The Viking Press. 1944.

DR. KROBER visited Peru in March and April of 1942, journeying from Lima southward to Arequipa and northward to Lambayeque. His book, however, covers a much larger scope, for it deals with all the outstanding problems of Peruvian archeology and with the current ideas of the leading field workers. In 1925 and 1926, the author had conducted excavations in both northern and southern Peru, on which and on museum collections he based various detailed studies. These were partly prepared in collaboration with Drs. Gayton, O'Neil and Strong; they were published by the University of California and Field Museum. His present work not only continues previous studies but is a résumé of the current state of Peruvian archeology.

Dr. Krober points out that some sixty cultural phases are now recognized in Peru. Of these, he has assembled fifty in a chronological tabulation, which presents the cultural sequences as they are now known in eight different areas. The author remarks that such diagrams tend to crystallize into dogma, but they are extremely useful to the non-specialist and specialist alike.

About half of the sixty cultural phases, it is inter-

esting to note, have been discovered or distinguished since 1910. Dr. Krober comments that "the doubling of archeological operative categories in one working life-time is a sign of the healthy growth of Peruvian archeology."

While much of the text necessarily is technical and detailed, major problems are also discussed. Perhaps the most important recent change in regard to Peruvian pre-history is that Uhle's concept of an early and primitive fishing population is no longer accepted. This culture, represented by massive incised pottery from Ancón and Supe, has been known for many years. Similar pottery, with regional variations, is now known at many other sites both on the coast and in the mountains, including the famous ruins at Chavín. Archeologists, by demonstrating the distribution and period of this culture, have a new and valuable tool for determining cultural sequence in northern Peru, comparable to the widespread Tiahuanaco and Inca cultures.

Dr. Krober has written a clear analysis of the famous Paracas culture, which he divides into two major chronological periods. These he places immediately before and after early aspects of Nasca art. He has also discussed in detail the stone carvings of Chavín and Sechín, which have been described by Drs. Bennett and Tallo in recent publications.

In regard to absolute dates of prehistoric culture in Peru, Dr. Krober is cautious in view of the lack of specific evidence. For some years it has been customary to ascribe early cultures to the early centuries of the Christian era. The author writes, "My only feeling is that many of the suggested dates have been put needlessly far back, which of course is always more impressive." The reviewer voices his agreement with this statement, especially in the light of material unearthed in Peru in 1943 and 1944. Similar downward revisions of time estimates have been necessary in regions where knowledge of absolute chronology has made great advances, such as the southwestern United States and the Maya area.

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SPECIAL ARTICLES

THE DEGRADATION OF CYSTINE PEPTIDES BY TISSUES[1]

SEVERAL years ago Bergmann, *et al.*, suggested that amino acids might be enzymatically degraded while they are yet in peptide combination, yielding by an $\alpha\beta$ dehydrogenation the corresponding "dehydropeptide."[2] Several types of dehydropeptides were synthesized, and with the discovery that extracts of swine kidney could effect the hydrolysis of such peptides the possibility that the latter were indeed biological intermediates seemed valid.[3] The enzyme responsible for this hydrolysis was named dehydropeptidase, and it was shown to be distinct from those peptidases which act upon the peptides of the naturally occurring, saturated amino acids, *i.e.*, aminopeptidase, carboxypeptidase, etc. The significant products yielded by the action of dehydropeptidase were ammonia and the corresponding α keto acid, both derived from the dehydrogenated amino-acid moiety.[3]

The concept that amino acids in peptide combination could be enzymatically attacked was a bold and discerning stroke on Bergmann's part, for it suggested that the catabolism of proteins could begin at the polypeptide stage. Bergmann did not commit himself on what the possible natural precursors of the dehydropeptides could be. We suggest that the peptides of cystine (or cysteine) serve in this capacity, yielding by desulfuration the corresponding dehydropeptide, and our evidence follows.[4]

There is present in certain mammalian tissues an enzyme which rapidly degrades cystine (aerobically or anaerobically) to pyruvic acid, ammonia and hydrogen sulfide.[5] We have found that certain peptides of cystine were degraded to the same products under similar conditions.[6] New data on such peptides, their derivatives and related substances digested with rat liver extracts are given in Table 1. The data reveal (1) that the cystine moiety of the vulnerable peptides must have either a free amino or a free carboxyl group (thus be exopeptides), since glutathione and the diketopiperazines are not attacked; (2) that the sulfur atom in an otherwise available substrate must be free since S-benzylcysteine is not attacked; (3) that the ammonia and pyruvic acid found in digests of the vulnerable peptides such as dichloracetylcystine must have been evolved from the cystine moiety subsequent to the rupture of the peptide bond; (4) that cystine

[1] Our interest in the cystine-degrading system arose from the fact that although it is found in certain normal tissues, it is the only intracellular enzyme that we have encountered which appears to be completely absent in every kind of cancerous tissue studied, *c.f.*, J. P. Greenstein, Gibson Island Conference on Cancer, 1944.

[2] M. Bergmann, V. Schmitt and A. Miekeley, *Zeits. physiol. Chem.*, 187: 264, 1930; M. Bergmann and K. Grafe, *Zeits. physiol. Chem.*, 187: 187, 1930.

[3] M. Bergmann and H. Schleich, *Zeits. physiol. Chem.*, 205: 65, 1932 and 207: 235, 1932.

[4] J. P. Greenstein and F. M. Leuthardt, *Jour. Nat. Cancer Inst.*, 5: no. 3, 1944 (in press). Cystine and cysteine, free or in peptide form, yield identical products, and to avoid repetition we shall use only the former term.

[5] C. V. Smythe, *Jour. Biol. Chem.*, 142: 387, 1942.

[6] J. P. Greenstein, *Jour. Nat. Cancer Inst.*, 3: 491, 1943.