tralopithecines being regarded as a branch remote from human ancestry). The effects of natural selection on race formation are discussed by E. B. Ford (in Lepidoptera), P. M. Sheppard (snails, moths, Drosophila), D. Lack (variations in fecundity in races of birds), H. N. Southern (mimicry in cuckoos), and E. Mayr (continental and island races in birds). The evolution of behavior, primarily in birds, is dealt with by J. Fisher, N. Tinbergen, and H. B. Cott; B. Rensch considers the relations between body size, brain structure, learning ability, and behavior in mammals and other vertebrates. The bearing on evolutionary problems of certain aspects of sensory physiology is considered by E. N. Willmer, J. Z. Young, and B. Rensch. J. B. S. Haldane and E. Mayr examine the properties of some genetic population structures. R. A. Fisher comments on the history of the theory of natural selection. The lone paleontologist, T. S. Westall, considers the correlations (or the lack thereof) between the periods of intense mountain building and biological evolution.

It is impossible in a review to do justice to the wealth of ideas discussed in the symposium. One of the controversial issues to which considerable space is devoted concerns the role in evolution of random changes in gene frequencies, referred to as the genetic drift or Sewall Wright's effect. In his excellent paper, Sheppard presents evidence that variations in seemingly indifferent characters, such as the coloration of shells in certain snails, have considerable adaptive significance and are subject to strong selection. But he concludes that: "Thus Wright's concept of drift, though frequently invoked as an important agent in evolution, must be judged of negligible significance as compared to selection." Now, this is a wrong way to state the problem. Genetic drift is not an alternative to selection; its importance lies precisely in its interaction with selection and other directive agents of gene frequency change. The futility of much evolutionary speculation in the past resulted from attempts to picture evolution as caused by some one agent—by mutation, or selection, or hybridization, and so forth. Perhaps the most important advance in the evolutionary thought was the realization that it is the coaction of many agents that brings about evolutionary changes. What kind of change occurs in a population depends on the magnitudes of all the "forces" impinging upon it, as well as on the genetic structure of the population determined by its previous history. Haldane is certainly right when he insists on this quantitative approach to the study of population structure and evolution.

How important may be random changes in gene frequencies is shown by Mayr, who points out that widespread, common, and polymorphic species which inhabit continental areas are usually conservative in the evolutionary sense. And yet geographically peripheral populations of such species, isolated on small islands or by other means, often show striking divergence from the main body of the species, forming clear-cut races or derived species. Mayr ascribes the relative conservatism of continental populations to continuous migration of genes throughout the distribution range, which maintains a high degree of coadaptation of the components of their gene pools. The situation changes when a small number of individuals, perhaps a single pair, forms an isolated colony. The foundation stock of such a colony will almost always contain only a fraction of the genetic variability present in the parental population. This will necessitate a reintegration of the gene pool to arrive at a new adaptively coherent genetic system. An isolated population may thus give rise to a new species, even though it inhabits an environment which is not greatly different from that in which the original population lives. But it may also enter a new ecological niche, and thus produce a novel adaptive type. It would be interesting to examine from Mayr's point of view the remarkable island races of some British butterflies described by E. B. Ford in the same symposium. The origin of these races is not convincingly accounted for either by selection alone or by genetic drift alone, but it may be due to interaction of these factors in populations in which migration of genes is limited or excluded.

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Gas Dynamics of Thin Bodies. F. I. Frankl and E. A. Karpovich. Trans. from the Russian by M. D. Friedman. Interscience, New York–London, 1954. viii + 175 pp. Illus. $5.75.

The subject of this slender monograph is of interest to some aeronautical engineers but is unlikely to attract mathematicians or physicists. The basic equations of the theory of ideal fluids are linearized, and thereafter follow various further assumed simplifications. The method is well known; the purpose of this monograph is to present the minute details in certain special cases of aeronautical interest.

The work is typical of much now published in Russia. Hastily thrown together in a miserable ragged style, without any attempt to survey the entire literature of the subject, it presents recent Russian methods not easily available otherwise and serves as a unique handbook for those whose particular field is its subject. It is pointless to criticize the details: each person will esteem this book much as he does other recent Russian monographs, according to his personal tastes.

The topics include motion of an elongated body of revolution at subsonic or supersonic speed, accelerated motion, the theory of thin or thick wings of infinite or finite span, unsteady motion of wings and the theory of propellers, and conical flow and its generalizations. Each of these has various special cases which can be combined in many ways: symmetric or unsymmetric bodies, zero or non-zero angle of attack, and so forth. Among the names mentioned are Chaplygin, Prandtl, Busemann, Ackeret, Sedov, Smirnov, Sobolev, Christianovich, Krasilshchikova (here the

translator adds Evvard), Falkovich, Puckett, Khaskind, Frankl, Gurevich, Karpovich, and Hayes. The work is dense with calculations, but the mathematics is simple and within reach of the aeronautical engineer. The "approximations" are heuristic and formal, there is no trace of what a mathematician would regard as a precise theory. The authors make no mention of the extensive literature arguing in favor of retaining terms they throw away or of throwing away terms they retain, as is usual among perturbationists. Thus they give the linearized theory an appearance of finality which would be destroyed by any more catholic presentation of the field.

The translator tells us he has made a literal translation, "with no effort . . . to impose the translator's style on the author's intentions." However, he has added at least one reference (to himself) and one plate without any special notice; what else he may have changed we can only guess. In some places he appears to preserve the Russian word order, and surely we cannot blame the author for "make $\partial\phi/\partial t$ to vanish" (p. 5), "the formula for the pressure is derived, now" (pp. 6–7), "to show that the wave equations, is satisfied" (pp. 11–12), "the body generatrices" (p. 22), and so forth, not to mention the many misspellings.

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Protein Metabolism. R. B. Fisher. Methuen, London; Wiley, New York, 1954. ix + 198 pp. Illus. $2.50.

This interesting and thoughtfully written little book is the latest member of the excellent series known as *Methuen's Monographs on Biochemical Subjects.* Like its predecessors, it is pocket-sized and very convenient to carry about for perusal at odd times.

It is the stated purpose of the author to "re-view" the essential knowledge of protein metabolism and to remind the reader of the possibility of reinterpreting discovered facts in the light of later findings. He is interested in redirecting the student to some of the older literature and is opposed to the rather popular notion that what is latest is always best.

There are seven essays, all but one accompanied by a conclusion. There is a general conclusion at the end of the book. The essays are: I, "Digestion and absorption of protein"; II, "The overall picture of protein metabolism"; III, "The metabolic significance of specific enzymes"; IV, "General aspects of the metabolism of the amino acids"; V, "The use of isotopes in the study of protein metabolism"; VI, "Metabolic and endocrine interactions in protein metabolism"; and VII, "The nutritive value of proteins." In each case the author undertakes to question current hypotheses, particularly in those cases where the physiological point of view seems to have been lost sight of or neglected. His central theme is that protein metabolism must be considered to be the metabolism of the amino acids in concert and that, of the variety of chemical and physiological factors which are kept in balance, no one can be neglected indefinitely.

Among the questions raised are included the examples that follow. It is my opinion that reappraisals which are the heart of this small volume may well stimulate future studies. Fisher, who is demonstrator in biochemistry, University of Oxford, considers that it is not firmly established that the "currency" of protein metabolism is amino acids. There is a distinct possibility that small peptides may fulfill such a role. There is no complete assurance that amino acids only are the end products of digestive proteolysis. The time required for complete digestion of proteins by protease action *in vitro* seems to be at variance with any assumption of complete hydrolysis.

It is the author's view that protein synthesis involves not only competition for precursors but also secondary physiologic influences which affect the synthetic process. In the light of present data, transamination, deamination, and the urea cycle seem to be somewhat lacking insofar as detailed knowledge of protein catabolism is concerned. The author is quite skeptical of experiments which have involved feeding a high level of a single amino acid along with an already adequate protein intake. He feels that studies with isotopically labeled amino acids have led to real advances in understanding in spite of certain difficulties of interpretation.

This book is to be recommended as a stimulating and brief analysis of a very complex active research area.

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Structure Reports for 1950. vol. 13. A. J. C. Wilson, Gen. Ed.; N. C. Baenziger (Metals), J. M. Bijvoet (Inorganic compounds), and J. Monteath Robertson (Organic compounds), Section eds. Oosthoek, Utrecht, Holland, 1954 (For the International Union of Crystallography. viii + 643 pp. Illus. $21.50.

This is the fourth volume of the series to be published, and it is the latest step in the process of catching up with publications of structural interest during the period since the last issue of the *Strukturbericht,* vol. VII for 1939. Like most workers concerned with structural studies, I turn to each of these reports as they appear to see not only what articles of importance I may have overlooked but also whether I have fully appreciated all important points in the articles I have supposedly read. For these are not abstracts in the ordinary sense; they aim to survey structural work so fully that nothing further would be gained by consulting the original papers. Indeed, these reports may go even further, for the abstractors sometimes give their own comments on the work. For example, their own calculations of interatomic separations may be compared with those in the literature. This is a wonderful service for all structure analysts, and one wonders whether the editors and their col-