must be ingested instead of 100, if that represents the starvation requirement. Rubner[25] explains that the cells of the body do not require more energy after meat ingestion than in starvation, but that the heat produced by a preliminary cleavage of proteid into dextrose on the one hand, and into a nitrogen containing rest on the other, while yielding heat to the body does not furnish the actual energy for the vital activities of the protoplasm. This is furnished principally by the dextrose derived from the proteid. Although it is necessary to abandon the older theory which pronounces glycogen (or dextrose) a direct cleavage product of proteid, still the explanation of Rubner remains tenable if interpreted in the newer light. If the energy requirement of the cell remains constant at 100, even after the ingestion of 140 calories of proteid, then 71.4 per cent. of the total heat value of the proteid is the quantity actually used for the vital processes. Since it has been shown in the writer's laboratory that meat proteid yields 58 per cent. of dextrose in metabolism, it may be calculated that 52.5 per cent. of the total energy of proteid may be available for the cells in the form of sugar. A balance of 19 per cent. must be obtained from other compounds, while 28.5 per cent. of the total heat value is wasted as heat without ever having been brought into the service of the life processes of the cells. Perhaps this 28.5 per cent. of heat loss represents the quantity produced by the cleavage of proteid into amino bodies and the denitrogenization of these radicles.

The constancy of the energy requirement in metabolism makes difficult the explanation of the action of the various ferments found in the body. These are of two varieties, hydrolytic and oxidizing, but these from the very principles of our knowledge must be subservient to the requirement of the living cells, and not themselves masters of the situation, as, for example, they are in the autolysis of dead tissue. It seems to be the requirement of the mechanism of cell activity which determines metabolism, and not primarily the action of enzymes, whose influence appears to be only intermediary.

Friedenthal[26] shows that proteid, colloidal carbohydrates, fats and soaps are not oxidizable in the cellular fluids without previous hydrolytic cleavage. After hydrolysis, however, the oxidases may effect an oxidation of the smaller molecules. The necessity of the hydrolytic ferment is seen in the non-combustion of dextrose after the extirpation of the pancreas, the organ by which the ferment is supplied. Oxygen and the oxidases are present in ample quantity, but the sugar is not burned unless it be broken by its specific ferment. In the meantime the cell avails itself of a compensatory energy supply from other sources. It is impossible to apply anything similar to Ehrlich's side-chain theory to this condition of affairs, for the metabolism does not depend upon the satisfaction of chemical affinities, but rather upon a definite law of utilization of energy equivalents.

However clearly formulated the laws of metabolism may be, and many of them are as fixed and definite as are any laws of physics and chemistry, still the primary cause of metabolism remains a hidden secret of the living bioplasm.

Graham Lusk.

University and Bellevue Hospital Medical College.

[25] Rubner, 'Gesetze des Energieverbrauchs,' 1902, p. 380.

[26] Friedenthal, 'Verhandlungen der Berliner Physiologischen Gesellschaft,' *Archiv für Physiologie*, 1904, p. 371.

SCIENTIFIC BOOKS.

Notes on Anthropoid Apes. By the Hon. Walter Rothschild.

This paper, in the last number of the *Proceedings of the Zoological Society of London*

(1904, Vol. II., Pt. II., 413–440), is based in the main upon recent studies by Professor Matschie, published in the *Sitz. Ges. Naturf. Freunde*.

A review of the systematic portion of Mr. Rothschild's paper could not be profitably undertaken at present, at least by an American zoologist, for lack of material by which values could be estimated, and still more by reason of the absence from his paper of almost all details in support of its conclusions except a few of dubious significance. The doubt may be expressed, however, whether even the German naturalist, though his material has much exceeded that ever before brought together, has had anything like a sufficient amount to establish the nature and the taxonomic value of many of his characters. One point which may be briefly noticed is Matschie's proposal, adopted by Rothschild (p. 413), that the gibbons should form a family, Hylobatidæ, quite apart from the other anthropoids. It appears to me that nothing could be further from sound principles of classification. By reason of their somewhat intermediate anatomical structure, the gibbons might, perhaps, be used to break down the separation of anthropoids and old-world monkeys into two families, but they are far too closely allied to the first in all distinctive characters, to be added as a third group in the series.

Reference may also be made here to the biological improbability of four subspecies of orang, each presenting the same dimorphic forms (p. 434).

The changes in nomenclature, proposed chiefly by Matschie, are so serious in their results that they need examination. It is proposed to transfer the generic name *Simia* Linn. from its time-worn association with the orang to the chimpanzees, and to apply to the former the name *Pongo* Lacép. Now a complete reversal in the relation of a generic and specific name a century and a half old, with the upsetting of all depending nomenclature, should be shown to be unavoidable before it is proposed. Is it so here? The contention is that it results from taking the tenth edition of the 'Systema Naturæ' (1758) as the starting point, instead of the twelfth edition (1766), for the reason that *Simia satyrus* of the tenth was based on the *Satyrus indicus* of Tulpe (1641), which Mr. Rothschild holds to be so unmistakably a chimpanzee that 'we can even distinguish the exact race to which it belongs.'

The whole question, therefore, hangs on the certainty with which this animal can be identified. To me it appears doubtful, as it did to Hartmann, what animal Tulpe really meant. He calls it *Satyrus indicus* and gives the habitat as 'Africa, Asia.' The '*crinibus nigris*' of his description is the one character to distinguish it from the red orang, but it does not serve to distinguish one species of chimpanzee from another, or more than doubtfully from a young gorilla. Turning to Tulpe's figure the zoologist of experience with living anthropoids is likely to recognize much more resemblance to the orang than to the chimpanzee in the head, the small ear, the protuberant paunch, the size of the great toe and in the whole attitude of the animal.

Linnæus had really never seen any of these apes and his names are based on statements of other authors who were not able to differentiate the red ones of the Oriental region from the black ones of the Ethiopian, and his genus *Simia* of the tenth edition does not rest surely —to quote the American code—upon 'a designated recognizable species * * * or plate or figure.' In the twelfth edition his *Simia satyrus* is, without question, the orang, the chief reference being to Edwards's plate 213 (1758), which being colored leaves no doubt as to which animal is figured. The fact is that *Simia* Linn. is merely a composite of all the monkeys known to that author, and has with others of his genera been imposed upon literature more by reverence for his name than through any exact application borne by them. This being true in many cases, and *Simia satyrus* of the tenth edition not being certainly identifiable, rather than overturn the whole nomenclature of two genera, or even worse to reverse it, it seems quite within legitimate practice to regard it as a *nomen nudum* as far as the tenth edition is concerned, and let it take date from its first unquestioned use in the twelfth.

An unfortunate result of the contrary view held by the two authors is that *Pongo* Lacép. (1799) takes the place of *Simia* for the orang. Unfortunate, for however much the proper use of this word has been confused by later authors, old Andrew Battell, in 'Purchas' made it clear that the native name *pongo* belongs to the gorilla, and while it is true that some of the codes now in use do not consider that grievous misapplication in meaning is cause for removal, it may be doubted if any rule which serves to perpetuate error in fact stands on a lasting base where scientific exactness is the object.

Simia satyrus being transferred to a species of chimpanzee, the proper name for the orang, according to Mr. Rothschild (p. 421), is *Pongo pygmæus* (Linn.). The paper of Linnæus's understudy, Hoppius, in the 'Amœnitates Academicæ' (1763), which is the reliance for this, is not really binominal and should not be considered. The first available use of *pygmæus* is in Schreber (1796), where it is based on Tyson's excellent figure of a chimpanzee. This is adopted by Rothschild for one of the chimpanzees, as *Simia pygmæa* (Schr.); the orang being *Pongo pygmæus* (Linn.)—an ill-judged and indefensible confusion.

All these lamentable changes may be avoided by the manner of treatment I have suggested, which appears to me to be quite within the rules. Present synonymy will be undisturbed and an appalling amount of confusion will be escaped. How great this is will be seen on attempting to correlate Mr. Rothchild's nomenclature with some known species. The only change required is that *Pan* Oken (1816) seems necessary for the chimpanzee, but this does not entail any alteration in specific names.

If it is to be regretted that Mr. Rothschild (p. 421) has followed Matschie so closely as to continue the erroneous date of '*Satyrus* Lesson, 1799'—which should be 1840—it is, at least, unalloyed gratification to be assured (p. 440) that the distinguished author and patron of zoological science is prepared to lead continental and American zoologists in the campaign for a system of pure trinomials.

Arthur Erwin Brown.
Zoological Gardens, Philadelphia,
May 27, 1905.

SCIENTIFIC JOURNALS AND ARTICLES.

The American Naturalist for June contains the following articles:

E. W. Berry: 'Fossil Grasses and Sedges.

H. W. Rand and J. L. Ulrich: 'Posterior Connections of the Lateral Vein of the Skate.'

H. W. Rand: 'The Skate as a Subject for Classes in Comparative Anatomy; Injection Methods.'

T. H. Romeiser: 'A Case of Abnormal Venous System in *Necturus maculatus*.'

R. H. Howe, Jr.: 'Sir Charles Blagden, earliest of Rhode Island Ornithologists.'

C. R. Eastman: 'The Literature of Edestus.'

SOCIETIES AND ACADEMIES.

THE BOTANICAL SOCIETY OF WASHINGTON.

The twenty-ninth regular meeting of the Botanical Society of Washington was held at the Portner Hotel, May 27, 1905. The following papers were presented:

Evolutionary Status of the Laminariaceæ: Walter T. Swingle.

Mr. Swingle's paper was illustrated by specimens from the algal herbarium of Mrs. W. T. Swingle. It was pointed out that of the twenty-two genera belonging to the true Laminariaceæ (*Corda* and *Adenocystis* being excluded) twelve (or over one half) are limited to the Pacific coast of the United States, from Lower California to British Columbia. In all, sixteen genera occur within these limits, while two more occur in Alaska and one more in New England, making nineteen genera in all from the United States territory in North America, or *over four fifths of the known genera.* In this territory there are fifty-one species, or almost exactly half of the one hundred and five species now known from the whole world.

The Laminariaceæ were shown to be cold-water algæ and are limited in their distribution chiefly by the summer temperatures of the sea water. The family originated in the