It was found that the second form of experiment gave the most uniform results; the method by cooling being less accurate, owing to currents of air in the room, etc. The results are embodied in the following Table:—

(Rate of Heating from 25° to 50°.)

TABLE I.

| Pressure. | Temperature. | Seconds occupied in rising each 5°. | Total number of seconds occupied. |
|---|---|---|---|
| 760 millims. | 25° | 0 | 0 |
| | 25 to 30 | 15 | 15 |
| | 30 to 35 | 18 | 33 |
| | 35 to 40 | 22 | 55 |
| | 40 to 45 | 27 | 82 |
| | 45 to 50 | 39 | 121 |
| 1 millim. | 25° | 0 | 0 |
| | 25 to 30 | 20 | 20 |
| | 30 to 35 | 23 | 43 |
| | 35 to 40 | 25 | 68 |
| | 40 to 45 | 34 | 102 |
| | 45 to 50 | 48 | 150 |
| 620 M.* | 25° | 0 | 0 |
| | 25 to 30 | 20 | 20 |
| | 30 to 35 | 23 | 43 |
| | 35 to 40 | 29 | 72 |
| | 40 to 45 | 37 | 109 |
| | 45 to 50 | 53 | 162 |
| 117 M. | 25° | 0 | 0 |
| | 25 to 30 | 23 | 23 |
| | 30 to 35 | 23 | 46 |
| | 35 to 40 | 32 | 78 |
| | 40 to 45 | 44 | 122 |
| | 45 to 50 | 61 | 183 |
| 59 M. | 25° | 0 | 0 |
| | 25 to 30 | 25 | 25 |
| | 30 to 35 | 30 | 55 |
| | 35 to 40 | 36 | 91 |
| | 40 to 45 | 45 | 136 |
| | 45 to 50 | 67 | 203 |
| 23 M. | 25° | 0 | 0 |
| | 25 to 30 | 28 | 28 |
| | 30 to 35 | 33 | 61 |
| | 35 to 40 | 41 | 102 |
| | 40 to 45 | 55 | 157 |
| | 45 to 50 | 70 | 227 |
| 12 M. | 25° | 0 | 0 |
| | 25 to 30 | 30 | 30 |
| | 30 to 35 | 37 | 67 |
| | 35 to 40 | 41 | 108 |
| | 40 to 45 | 58 | 166 |
| | 45 to 50 | 86 | 252 |
| 5 M. | 25° | 0 | 0 |
| | 25 to 30 | 38 | 38 |
| | 30 to 35 | 43 | 81 |
| | 35 to 40 | 54 | 135 |
| | 40 to 45 | 71 | 206 |
| | 45 to 50 | 116 | 322 |
| 2 M | 25° | 0 | 0 |
| | 25 to 30 | 41 | 41 |
| | 30 to 35 | 51 | 92 |
| | 35 to 40 | 65 | 157 |
| | 40 to 45 | 90 | 247 |
| | 45 to 50 | 165 | 412 |

There are two ways in which heat can get from the glass globe to the thermometer—(1) By radiation across the intervening space; (2) by communicating an increase of motion to the molecules of the gas, which carry it to the thermometer. It is quite conceivable that a considerable part, especially in the case of heat of low refrangibility, may be transferred by "carriage," as I will call it to distinguish it from convection, which is different, and yet that we should not perceive much diminution of transference, and consequently much diminution of rate of rise with increased exhaustion, so long as we work with ordinary exhaustions up to 1 millim. or so. For if, on the one hand, there are fewer molecules impinging on the warm body (which is adverse to the carriage of heat), yet on the other the mean length of path between collisions is increased, so that the augmented motion is carried further. The number of steps by which the temperature passes from the warmer to the cooler body is diminished, and accordingly the value of each step is increased. Hence the increase in the difference of velocity before and after impact may make up for the diminution in the number of molecules impinging. It is therefore conceivable that it may not be till such high exhaustions are reached that the mean length of path between collisions becomes comparable with the diameter of the case, that further exhaustion produces a notable fall in the rate at which heat is conveyed from the case to the thermometer.

*M=millionth of an atmosphere.

The above experiments show that there *is* a notable fall, a reduction of pressure from 5 M. to 2 M. producing twice as much fall in the rate as is obtained by the whole exhaustion from 760 millims. to 1 millim. We may legitimately infer that each additional diminution of a millionth would produce a still greater retardation of cooling, so that in such vacua as exist in planetary space the loss of heat—which in that case would only take place by radiation—would be exceedingly slow.

## PROFESSOR HUXLEY ON EVOLUTION.

At a recent meeting of the Zoological Society, among the papers read was one by Professor Huxley on the application of the laws of evolution to the arrangement of the vertebrata, and more particularly mammalia. The illustrations adduced were those of the history of the horse, principally, so far as is known, from the work of Professor Marsh on the Eocenes of North America. The announcement of the paper had drawn together an unusually large attendance, as it was expected that the marshalling of the facts in Professor Huxley's hands would have great interest in practically substantiating the theory of evolution, which, though foreshadowed by others, took practical shape in the work of Darwin twenty-one years ago.

Professor Huxley began by saying:—There is evidence, the value of which has not been disputed, and which, in my judgment, amounts to proof, that between the commencement of the tertiary epoch and the present time the group of the equidæ has been represented by a series of forms, of which the oldest is that which departs least from the general type of structure of the higher mammalia, while the latest is that which most widely differs from that type. In fact, the earliest known equine animal possesses four complete sub-equal digits on the fore foot, three on the hind foot; the ulna is complete and distinct from the radius; the fibula is complete and distinct from the tibia; there are 44 teeth, the full number of canines being present, and the cheek-teeth having short crowns with simple patterns and early-formed roots. The latest, on the other hand, has only one complete digit on each foot, the rest being represented by rudiments; the ulna is reduced and partially anchylosed with the radius; the fibula is still more reduced and partially anchylosed with the tibia; the canine teeth are partially or completely suppressed in the females; the first cheek-teeth usually remain undeveloped, and when they appear are very small; the other cheek-teeth have long crowns, with highly complicated patterns and late-formed roots. The equidæ of the intermediate ages exhibit intermediate characters. With respect to the interpretation of these facts two hypotheses and only two, appear to be imaginable. The one assumes that these successive forms of equine animals have come into existence independently of one another. The other assumes that they are the result of the gradual modification undergone by the successive members of a continuous line of ancestry. As I am not aware that any zoologist maintains the first hypothesis, I do not feel called upon to discuss it. The adoption of the second, however, is equivalent to the acceptance of the doctrine of evolution so far as horses are concerned, and in the absence of evidence to the contrary, I shall suppose that it is accepted. Since the commencement of the eocene epoch, the animals which constitute the family of the equidæ have undergone processes of modification of three kinds:—1, there has been an excess of development of one part of the oldest form over another; 2, certain parts have undergone complete or partial suppression; 3, parts originally distinct have coalesced. Employing the term "law" simply in the sense of a general statement of facts ascertained by observation, I shall speak of these three processes by which the eohippus form has passed into equus as the expression of a three-fold law of evolution. It is of profound interest to remark that this law or generalized statement of the nature of the ancestral evolution of the horse, is precisely the same as that which formulates the process of individual development in animals generally, from the period at which the broad characters of the group to which an animal belongs are discernible onwards. After a mammalian embryo, for example, has taken on its general mammalian characters, its further progress towards its special form is affected by the excessive growth of one part or relation to another, by the arrest or suppression of parts already formed, and by the coalescence of parts primarily distinct. This coincidence of the laws of ancestral and individual development creates a strong confidence in the general validity of the former, and a belief that we may safely employ it in reasoning deductively from the known to the unknown. The astronomer who has determined three places of a new planet calculates its place at any epoch, however remote; and, if the law of evolution is to be depended upon, the zoologist who knows a certain length of the course of that evolution in any given case may with equal justice reason backwards to the earlier but unknown stages. Applying this method to the case of the horse, I do not see that there is any reason to doubt that the eocene equidæ were preceded by mesozoic forms, which differed from eohippus in the same way as eohippus differs from equus. And thus we are ultimately led to conceive of a first form of the equine series, which, if the law is of general validity, must need have been provided with five sub-equal digits on each plantigrade foot, with complete sub-equal antebrachial and crural bones, with clavicles, and with, as at present, 44 teeth, the cheek-teeth having short crowns and simple ridged or tuberculated patterns. Moreover, since Marsh's investigations have shown that the older forms of any given mammalian group have less developed cerebral hemispheres than the later, there is a *primâ facie* probability that this primordial hippoid had a low form of brain. Further, since the existing horse has a diffuse allantoic placentation, the primary form could not have presented a higher, and may have possessed a lower condition of the various modes by which the fœtus derives nourishment from the parent. Such an animal as this, however, would find no place in any of our systems of classification of the mammalia. It would come nearest to the lemuroidea and the insectivora, though the non-prehensile pes would separate it from the former, and the placentation from the latter group. A natural classification is one which associates together all those forms which are closely allied and separates them from the rest. But, whether in the ordinary sense of the word "alliance," or in its purely morphological sense, it is impossible to imagine a group of animals more closely allied than our primordial hippoids are with their descendants Yet, according to existing arrangements, the ancestors would have to be placed in one order of the class of mammalia, and their descendants in another. It may be suggested that it might be as well to wait until the primordial hippoid is discovered before discussing the difficulties which will be created by its appearance. But the truth is that that problem is already pressing in another shape. Numerous "lemurs," with marked ungulate characters are being discovered in the older tertiaries of the United States and elsewhere; and no one can study the more ancient mammals with which we are already acquainted without being constantly struck with the insectivorous characters which they present. In fact, there is nothing in the dentition of either primates, carnivores, or ungulates, which is any means of deciding whether a given fossil skeleton, with skull, teeth, and limbs almost complete, ought to be ranged with the lemurs, the insectivores, the carnivores, or the ungulates. In whatever order of mammals a sufficiently long series of forms has come to light, they illustrate the three-fold law of evolution as clearly, though, perhaps, not so strikingly, as the equine series does. Carnivores, artiodactyles, and perissodactyles all tend, as we trace them back through the tertiary epoch, towards less modified forms which will fit into none of the recognized orders, but come closer to the insectivora than to any other. It would, however, be most inconvenient and misleading to term these primordial forms insectivora, the mammals so-called being themselves more or less specialized modifications of the same common type, and only, in a partial and limited sense, representatives of that type. The root of the matter appears to me to be that the palæontological facts which have come to light in the course of the last ten or fifteen years have completely broken down existing taxonomical conceptions, and that the attempts to construct fresh classification upon the old model are necessarily futile. The Cuvieran method, which all modern classifiers have followed, has been of immense value in leading to the close investigation and the clear statement of the anatomical characters of animals. But its principle, the association into sharp logical categories defined by such characters, was sapped when Von Baer showed that, in estimating the likenesses and unlikeness of the animals, development must be fully taken into account; and if the importance of individual development is admitted, that of ancestral development necessarily follows. If the end of all zoological classification is a clear and concise expression of the morphological resemblances and differences of animals, then all such resemblances must have a taxonomic value. But they fall under three heads:—(1) those of adult individuals: (2) those of successive stages of embryological development or individual evolution; (3) those of successive stages of the evolution of the species, or ancestral evolution. An arrangement is "natural," that is, logically justifiable, exactly in so far as it expresses the relations of likenesses and unlikenesses enumerated under these heads. Hence, in attempting to classify the mammalia, we must take into account not only their adult and embryogenetic characters, but their morphological relations, in so far as the several forms represent different stages of evolution. And thus, just as the persistent antagonism of Cuvier and his school to the essence of Lamarck's teachings (imperfect and objectionable as these often were in their accidents) turns out to have been a reactionary mistake, so Cuvier's no less definite repudiation of Bonnet's "*échelle*" at the present day, the existence of a "scala animantium," is a necessary consequence of the doctrine of evolution, and its establishment constitutes, I believe, the foundation of scientific taxonomy. Many years ago, in my lectures at the Royal College of Surgeon, Is particularly insisted on the central position of the insectivora among the higher mammalia; and further study of this order and of the rodentia has only strengened my conviction that any one who is acquainted with the range of variation of structure in these groups possesses the key to every peculiarity which is met with in the primates, the carnivora, and the ungulata. Given the common plan of the insectivora and of the rodentia, and granting that the modifications of the structure of the limbs, of the brain, and of the alimentary and reproductive viscera which occur among them may exist and accumulate elsewhere, and the derivation of all eutheria from animals which, except for their diffuse placentation, would be insectivores, is a simple deduction from the law of evolution. I venture to express a confident expectation that investigation into the mammalia fauna of the mesozoic epoch will, sooner or later, fill up these blanks.

## RECENT DISCOVERIES RELATING TO THE DOUBLE STARS OF THE DORPAT CATALOGUE.

By S. W. Burnham.

The distinguished Russian astronomer, Struve, published in 1837 the results of a thorough examination of the heavens for the discovery of double stars between the north pole and 15° south declination. This great catalogue, *Mensuræ Micrometricæ*, included all the double stars within these limits known prior to the observations of Struve, mainly due to the researches of Sir William Herschel, and at the time of its publication presented all that was known on this subject of astronomy. The whole number of double stars catalogued and measured by Struve was about 3000. The superiority of the telescope used at Dorpat for this class of work, over the much larger reflectors employed by the Herschels, is repeatedly shown by the observations. Many of the Herschel pairs, observed with apertures from eighteen inches to four feet, were found by Struve with the 9.6-inch refractor to be really triple, one of the components being a close pair. When Sturve's great work was published, it seemed as though there was little left for subsequent observers to do except in the way of re-observing the Struve stars. So complete and systematic had been his scrutiny of the northern heavens, it was considered that new discoveries among the stars found by Struve to be single would necessarily be of rare occurrence, and particularly after the publication, in 1850, of the Pulkowa Catalogue of 500 stars, which comprised omitted stars and later discoveries, principally by Otto Struve, the successor of his father as Director of the new Imperial Observatory. This last mentioned catalogue was much more interesting, with respect to the class of stars it contained, than the other. The Pulkowa 15-inch refractor was in every respect superior to the Dorpat glass, as well as larger. Substantially all the wide and comparatively easy pairs had been collected in *Mensuræ Micrometricæ*, so that later discoveries were necessarily either very close pairs, or the components were very unequal, and, therefore, this catalogue furnishes a much larger proportion of binary and other interesting systems. In the twenty-five years following this epoch, the whole number of double star discoveries by all observers would not exceed fifty; but many important series of measures of the Struve stars were made by English, German and Italian astronomers, and this work was steadily continued at Pulkowa, resulting in showing the periods and motions of many of the more rapid binary systems, and the relations of other double stars.

That these catalogues were really very incomplete, with reference to the number of double stars actually existing, is apparent from the fact that the writer in the last ten years has discovered at least 900 new pairs, and more than half of them with a telescope greatly inferior in size to the smallest of the instruments used by the Russian astronomers. That there was left much that was new to discover in the Struve stars will appear from the number which have been again divided by later observers. In some instances, doubtless, the close pair was missed by Struve because it was single or much closer at that time, but certainly in the great majority of instances this is improbable, and the true explanation will probably be found in the improved defining power of the later refracting telescopes. For double star work more than any other, perfect definition is of the first importance. Something may be done in observing the moon, planets, nebulæ, etc., with a large instrument of poor definition, but for the discovery or measurement of close and difficult double stars it is practically useless. It should be mentioned as a fact that every star in the following table was discovered with a refracting telescope.

The follow ng list comprises all the stars of the Dorpat Catalogue where a closer component has been discovered since the observations of Struve. More than half of these

| No. | Σ | Star. | Struve's Pair. | New Companion. | Discoverer. |
|---|---|---|---|---|---|
| 1.. | 17 | ............ | 27″.06 | 2″.04 | Burnham |
| 2.. | 26 | .............. | 13 .29 | 0 .60 | O. Struve |
| 3.. | 39 | .............. | 19 .90 | 0 .40 | Dembowski |
| 4.. | 157 | .............. | 12 .40 | 0 .85 | Burnham |
| 5.. | 171 | .............. | 29 .69 | 3 .69 | Burnham |
| 6.. | 205 | $\gamma$ Andromedæ. | 10 .33 | 0 .50 | O. Struve |
| 7.. | 258 | .............. | 70 .30 | 1 .20 | Burnham |
| 8.. | 318 | 20 Persei..... | 14 .04 | 0 .34 | Burnham |
| 9.. | 366 | .............. | 48 .97 | 1 .99 | Burnham |
| 10.. | 439 | .............. | 23 .70 | 0 .40 | Burnham |
| 11.. | 610 | 7 Camelopardi | 25 .64 | 1 .24 | Dembowski |
| 12.. | 668 | $\beta$ Orionis..... | 9 .14 | 0 .2 ? | Burnham |
| 13.. | 692 | Orionis 82 .... | 34 .86 | 0 .48 | Burnham |
| 14.. | 707 | .............. | 27 .77 | 1 .11 | Burnham |
| 15.. | 721 | .............. | 24 .32 | 0 .46 | Burnham |
| 16.. | 808 | .............. | 16 .06 | 2 .60 | Dembowski |
| 17.. | 888 | .............. | 2 .83 | 0 .27 | Burnham |
| 18.. | 1019 | Canis Maj. 136 | 37 .84 | 6 .12 | Dembowski |
| 19.. | 1026 | Canis Maj. 139 | 17 .85 | 0 .48 | Burnham |
| 20.. | 1057 | .............. | 15 .87 | 0 .69 | Burnham |
| 21.. | 1097 | .............. | 29 .34 | 5 .93 | Dembowski |
| 22.. | 1179 | .............. | 19 .75 | 3 .76 | Burnham |
| 23.. | 1481 | .............. | 20 .20 | 0 .80 | Burnham |
| 24.. | 1516 | .............. | 7 .90 | 7 .61 | O. Struve |
| 25.. | 1780 | 86 Virginis (AC) | 26 .94 | 1 .61 | (AB) Burnham |
| | | | | 1 .72 | (CD) Burnham |
| 26.. | 1812 | .............. | 14 .02 | 0 .47 | O. Struve |
| 27.. | 2005 | Libræ 213 .... | 28 .54 | 1 .47 | (AB) Burnham |
| 28.. | 2214 | .............. | 19 .49 | 1 .43 | Dembowski |
| 29.. | 2220 | $\mu$ Herculis.... | 31 .09 | 0 .96 | Alvan Clark |
| 30.. | 2287 | .............. | 22 .33 | 1 .71 | Burnham |
| 31.. | 2306 | .............. | 12 .81 | 0 .95 | Dembowski |
| 32.. | 2342 | .............. | 28 .80 | 8 .86 | Burnham |
| 33.. | 2435 | (AC) | 10 .73 | 1 .43 | (AB) Burnham |
| | | | | 2 .90 | (CD) Howe |
| 34.. | 2479 | Cygni 4 ..... | 6 .72 | 0 .57 | Dembowski |
| 35.. | 2481 | .............. | 4 .03 | 0 .40 | Secchi |
| 36.. | 2535 | .............. | 26 .31 | 1 .22 | Dembowski |
| 37.. | 2538 | .............. | 52 .81 | 4 .37 | Burnham |
| 38.. | 2539 | .............. | 5 .60 | 4 .78 | Burnham |
| 39.. | 2549 | .............. | 22 .86 | 1 .93 | Burnham |
| 40.. | 2570 | .............. | 4 .16 | 0 .29 | A. G. Clark |
| 41.. | 2589 | $\zeta$ Sagittæ..... | 8 .77 | 0 .25 | A. G. Clark |
| 42.. | 2607 | Cygni 116.... | 3 .23 | 0 .3 | O. Struve |
| 43.. | 2630 | (AD) | 11 .30 | 6 .47 | (AB) Burnham |
| | | | | 7 .75 | (AC) Burnham |
| 44.. | 2657 | .............. | 11 .71 | 0 .60 | O. Struve |
| 45.. | 2690 | .............. | 14 .88 | 0 .56 | Dawes |
| 46.. | 2704 | $\beta$ Delphini.... | 35 .06 | 0 .20 | Burnham |
| 47.. | 2777 | $\delta$ Equulei.... | 37 .98 | 0 .35 | O. Struve |
| 48.. | 2793 | .............. | 26 .51 | 0 .56 | Burnham |
| 49.. | 2815 | .............. | 7 .50 | 0 .90 | Dembowski |
| 50.. | 2824 | $\kappa$ Pegasi...... | 11 .76 | 0 .27 | Burnham |
| 51.. | 2959 | .............. | 13 .77 | 8 .31 | Burnham |
| 52.. | 2966 | .............. | 30 .72 | 0 .41 | O. Struve |
| 53.. | 3130 | .............. | 2 .86 | 0 .3? | O. Struve |