

This question of the discordance between theoretical and observed results has been recently treated by Dr. Hausdorff, and greater importance has been given to the memoir by a review from Dr. Kempf, in a recently-issued *Vierteiljahrsschrift*; and this last, while traversing some of the views of the authors, is, on the whole, as satisfactory and reassuring as the views of Prof. Langley and Dr. Seeliger have been disturbing. Dr. Hausdorff, concerned to reconcile theory and observation, examines in the first place Laplace's theory with great accuracy to trace whether the deviations are due to any incompleteness in the theory itself. The principal result of this inquiry is to add a term of rather complex character to the simple formula to which Laplace reduced his expression for atmospheric absorption, but which avails nothing, either in actual practice or in offering a solution of the original difficulty. The new coefficients might be of use if their physical interpretation added anything to our knowledge of the atmosphere; but whether this be assumed of infinite or limited extent, the whole effect of the new introductions, even in small altitudes, can be represented by an alteration in the constant. How little improvement, if any, the consideration of terms of a higher order has had on the computed quantities, is shown by the following table, in which Müller's observed values of atmospheric absorption (M.) (expressed in logarithms) are compared with those of Hausdorff (H.), and the more simple expressions of Laplace (L.).

Z.D.	(M.)	(H.)	(L.)	(M)-(H.)	(M)-(L.)
0	0.000	0.000	0.000	0	0
20	0.004	.005	.005	- 1	- 1
40	0.024	.023	.024	+ 1	0
60	0.092	.076	.077	+ 16	+ 15
70	0.180	.146	.147	+ 34	+ 33
75	0.261	.215	.216	+ 46	+ 45
80	0.394	.350	.352	+ 44	+ 42
82	0.477	.447	.448	+ 30	+ 29
84	0.607	.597	.598	+ 10	+ 9
86	0.846	.856	.855	- 10	- 9
87.5	1.176	1.209	1.200	- 33	- 24

Practically identical figures result whether the atmosphere be supposed infinite or of limited extent.

The essential service that Dr. Hausdorff has rendered is to show that any considerable improvement in Laplace's theory is not probable. Dr. Kempf now renders a still greater service by showing that any improvement is not needed. He raises the question whether these observed discrepancies are not rather due to observation, and removable by more appropriate methods of discussing the observation. Dr. Kempf remarks that the only observations available for examination are those of Dr. Müller, whether made at Potsdam or on the Santis. Seidel is brushed aside with a scanty reference. Pickering and Pritchard do not get even this recognition. It would probably be objected to the latter that he had not observed below 75° Z.D., and it is only after this altitude is passed, that the deviations between observation and theory become of noticeable amount. But Prof. Pickering's observations of circumpolar stars at upper and lower culminations seem to be available, and should not be rejected without reason or excuse. But it may be urged that the real object of the inquiry is to explain Dr. Müller's observations, and from the connection that has long existed between Drs. Kempf and Müller, the remarks of the former become the more valuable, since it may be assumed that Dr. Müller is cognisant of the treatment that his observations have received at the hands of his coadjutor, and has tacitly acquiesced in the process.

Dr. Hausdorff has declined to use the Santis observations, because the corrections for light extinction are founded on Laplace's theory; but Dr. Kempf shows how the observations can easily be made to furnish results independent of any theory, and consequently the discrepancies between the observations and Laplace's values can be easily exhibited. Expressed as light ratios in logs., the unit corresponding to the third place of decimals, the differences Müller-Laplace are shown in the following little table, in which no regular systematic progress is noticeable.

Z.D.	(M.-L.)	Z.D.	(M.-L.)
18.6	... .. - 10	62.8	... .. - 10
29.3	... .. + 6	72.4	... .. + 9
37.2	... .. - 5	79.5	... .. + 3
44.6	... .. 0	82.9	... .. + 6
57.6	... .. + 3	85.8	... .. - 7

Not only are the differences small, but in no case do they exceed twice the probable error derived from the discussion of the observations themselves. Encouraged by this satisfactory agreement, Dr. Kempf applies himself to the Potsdam results, and recalls the fact that the extinction table, on which Dr. Hausdorff so much depends, is really the result of the combination of two separate processes of observation. The first part extends from the zenith to an altitude of 10°, and has been derived by comparing the light of Polaris with that of five different stars at every possible Z.D. between 0° and 80°. For greater Z.D., however, another series has been obtained by observing, on very clear nights, the differences of lustre of objects at their rising or setting up to some 10° of altitude, and deriving from the differences the amount of absorption at the various Z.D.s. Such a break in the continuity is perhaps regrettable, but to some extent unavoidable. Stars that are visible in the horizon do not reach the zenith of Potsdam within some 15°, and, of course, stars culminating near the zenith do not approach near enough to the horizon. Comparing each part of Müller's general extinction table with Laplace's theory, Dr. Kempf obtains the following result (M.-L.) I.; but, for very sufficient reasons, on discarding the last observation at 80° Z.D., which is evidently discordant, and affects the accuracy of the determination of the general coefficient of transmission, he obtains the second series marked (M.-L.) II., in which the agreement leaves very little to be desired.

Z.D.	(M.-L.) I.	(M.-L.) II.	Z.D.	(M.-L.) I.	(M.-L.) II.
20	... - 2	... - 2	72.5	... + 10	... + 5
40	... - 3	... - 4	75	... + 9	... + 2
50	... - 2	... - 3	77.5	... + 1	... - 7
60	... + 3	... 0	80	... - 17	... —
70	... + 9	... + 5			

Nearly as satisfactory is the comparison of the theory with the second part of the table, that below 80° Z.D. The greatest deviations, when expressed in magnitude, do not exceed 0.025 m., and any one acquainted with the difficulties attending photometric observations so close to the horizon, will rather be surprised that the agreement is so close, than tempted to cavil over the small discordances. In the first part, the coefficient of transmission is 0.81; in the second, 0.85: the two parts, therefore, correspond to different degrees of atmospheric transparency, and they cannot be represented by one and the same curve. The explanation offered by Dr. Kempf will probably command a general assent, and it will be admitted that he has made out his case, that Laplace's theory of absorption corresponds exactly to the actual conditions within the limits of accuracy at present attainable.

If Dr. Kempf has provided the true explanation, it is of little use to follow Dr. Hausdorff in his further investigations, based as they are on the entire conviction that Müller's extinction table is exact, and that the discordance between theory and observation is real. His attempts to devise new formulæ on various hypotheses are not very satisfactory, simply regarded as interpolation results; and his failure to represent Dr. Müller's figures more closely, tends to confirm the probability of Dr. Kempf's suggestion. Of course, by the extension of formulæ to an inconvenient length, and the introduction of a sufficiently large number of unknowns, derived from the observations themselves, a close agreement can be forced; but even then one may be driven to such inconvenient consequences as that the intensity of light at approximately the sea-level is greater than that at the top of a mountain 2500 m. high. On no supposition (and in some instances the ingenuity displayed in the construction of hypotheses is considerable) can a formula be found that more closely represents observation than does Laplace's; and though the author did not propose to himself to establish this fact, he has rendered no slight service to theoretical photometry by the practical confirmation his work affords.

W. E. PLUMMER.

#### ON CERTAIN VESTIGIAL CHARACTERS IN MAN.

SEEMING that Prof. Huxley, with his well-known candour, felt constrained to admit that the study of rudimentary or vestigial characters had done more than that of any other class of facts to produce general acceptance of the doctrine of evolution, and that at the same time he acknowledged the double-edged

nature of these characters, it is not out of place to appraise the evidential value of certain of them.

The direction of the hair-slope on three regions of the body, as bearing upon the simian ancestry of man, will be first considered.

(1) On the upper extremity of man the direction of the hair-slope, which may for the sake of brevity be called the *Human Type*, is as follows:—On the upper arm the slope is all downwards to the elbow, with a slightly oblique direction on the anterior surface. This direction appears to be the same as that in all the monkeys examined.

But on the fore-arm the *Human Type* is as follows:—On the flexor surface the stream of hair divides and passes obliquely to the radial and ulnar borders respectively, and to the carpus. On the extensor surface the slope continues on the radial side in a direction at right angles to the long axis of the limb, and gradually curls backwards over the posterior surface of the ulna, joining a corresponding “backwash” of the stream of hair from the ulnar border. Thus, on a small area amounting to about a fourth of the extensor surface, the united stream of hair passes directly to the elbow.

This description is based upon the examination of numerous fore-arms, hairy and non-hairy; of infants a few days old and three months old, of children from seven to fourteen years old, of adults male and female—among the adults five very hairy male subjects. In all of these fore-arms, as far as the scanty hair on some would allow one to observe it, there was very little departure from the *Human Type* as described. In the cases of infants, the hairs were very minute and required a lens to reveal them. The direction stated is easy to verify or to disprove; but it is surprising to find such a statement as occurs in “Darwin and after Darwin,” by the late Prof. Romanes, where, on page 89, he says, “again, in all men the rudimentary hair on the upper and lower arm is directed towards the elbow—a peculiarity which occurs nowhere else in the animal kingdom, with the exception of the anthropoid apes and a few American monkeys. . . .” With this statement Prof. Romanes and Prof. Drummond seem to have remained satisfied, though their own fore-arms, and those of every person they might have examined, would have told a different tale, either with or without the assistance of a lens. The statement of Prof. Romanes clearly refers to the permanent hair of the body, as shown by his illustrations, and not to the lanugo or temporary hair.

The direction of the hair-slope on the fore-arm of the anthropoid apes—the *Anthropoid Ape Type*—is certainly what is stated by Romanes and Drummond, viz. towards the elbow with a slightly lateral direction both on the flexor and extensor surfaces, except in the orang, in which the slope is all directly to the elbow. This is to be seen in all the anthropoid apes at the Zoological Society's Gardens, London, in the case of the gorilla, chimpanzee, and gibbon hoolock, and in the case of the orang at the Natural History Museum, South Kensington, where also the slope of the hair on the fore-arms of gorilla, chimpanzee, and gibbon is confirmed. St. George Mivart<sup>1</sup> mentions one species of gibbon, *Hylobates agilis*, where the *Human Type* appears to be exceeded in the *wrist-ward* direction. He says, “in *Hylobates agilis* all the hair of both these limb-segments is directed towards the wrist.”

This statement is not fully borne out by the examination of the specimens of *Hylobates agilis* at South Kensington.

In addition to these four genera of anthropoid apes, twenty-two other species of monkeys were examined as to the slope of the hair on the fore-arm, with the following results:—

A 19. *Catarrhine or Old World monkeys*, as follows:—

13. Human Type, viz.:—

*Cercocebus athiops*—*Barbary ape*—*Japanese ape*—*Cercopithecus campbelli*—*Cercopithecus ruber*—*Cercopithecus diana*—*Cercopithecus callitrichus*—*Cercopithecus lalandii*—*Cercopithecus griseo-viridis*—*Cynocephalus anubis*—*Macacus maurus*—*Arabian baboon*—*Cercopithecus abigulosus*.

(1) Anthropoid Ape Type, viz.:—*Cercopithecus cephus*.

(5) Partial Human Type inclining to Anthropoid Ape Type, viz.:—*Cynopithecus niger*—*Cercocebus fuliginosus*—*Macacus cynomolgus*—*Macacus rhesus*—*Macacus sinicus*.

<sup>1</sup> Encyclop. Brit., vol. ii. p. 157.

B (3) *Platyrrhine or New World monkeys*.

(2) Human Type, viz.:—

*Cebus fatuellus*—*Cebus monachus*.

(1) Anthropoid Ape Type, viz.:—*Ateles geoffroyi*.

Thus of twenty-two lower monkeys, Old World and New World, fifteen very closely resemble the human subject in this small morphological character, whereas all the anthropoid apes (one species of one genus excepted) are markedly different from the *Human Type*.

Such things ought not to be on the theory of the descent of man from the ape. They may not alone support the opposing theory, but they ought never to have found their way into valuable and popular books, being selected from a great array of so-called vestigial characters with a view to supporting the above theory.

(2) There is no reason why the direction of the hair-slope on the fore-arm should be studied in its vestigial character, any more than that on the *thigh*. On the *thigh* the *Human Type* is as follows: On the flexor surface the hair slopes in two streams to the outer and inner borders respectively, and towards the knee. At the upper third and outer side the slope takes a direction at right angles to the long axis. On the extensor surface the streams of hair which come from the borders coalesce and pass to the back of the knee. The Simian Type is oblique, and to the *pelvis*, i.e. in the favourite position of the monkey, when sitting on its haunches, the hair falls quite vertically downwards. This statement is based on the observation of the four anthropoid apes and twenty-seven other lower monkeys, including the twenty-two previously specified, thirty-one in all. There were found, out of these thirty-one specimens, ten partial exceptions, five American monkeys, and five lower Old World monkeys, such as baboons, Barbary ape, and Japanese ape. In these ten there was a slight resemblance to the *Human Type*, but not a vestige of resemblance in one of the anthropoid apes examined.

(3) A third region of the human body shows the divergence between the *Human Type* of hair-slope and the *Simian Type* even more strongly. On the dorsal surface of the trunk in man, in the erect posture, the hair slopes in the supra-scapular region inwards and at a right angle to the middle line, on approaching which it curls downwards. Below the spine of the scapula the same direction obtains until about the level of the angle, when the hair slopes *upwards* and inwards to a point over the transverse processes of the vertebrae, where it becomes horizontal and then curls sharply downwards, joins the stream of hair from the opposite side, and passes vertically downwards in the hollow over the vertebral spines. This Human Type I have found constant in children and adults, and it differs strikingly from that of all the apes and monkeys examined, in which, without exception, the hair slopes as nearly as possible vertically downwards, when the animal is sitting.

These remarks, calculated to disparage the value of the direction of the hair-slope on the human body as a “vestige” of his descent from the ape, may be met in two ways at least. In the first place, one may be reminded that it is not to the few existing anthropoid apes, “living fossils,” indeed, but to some unknown *dead* fossil apes of the Miocene period that we must look for the direct ancestry of man, and that the difference in the hair-slope pointed out is consequently unimportant. Perhaps it is. But the supposed resemblance was thought worthy of prominence in the works of evolutionists, and accordingly the ascertained divergence is worthy of not less prominence.

In the second place, the differing hair-slope on the *fore-arm*, *thigh*, and *back* of man and the anthropoid apes, may be explained by the possible influence which the greater weight of the long hair covering the bodies of apes would have in producing a *generally vertical direction* of hair-slope in the sitting posture. This posture doubtless is the one in which far the greater part of the life of the ape is spent, and a little consideration of the position of an ape in sitting, will show that gravitation would tend in the case of long-haired apes to produce the *Anthropoid Ape Type* on the fore-arm, thigh, and back. In the case of man, the action of gravity would be unable to influence the slope of his short rudimentary hairs. This suggestion of a possible cause contributing to the hair-slope on the bodies of apes has, however, no bearing on the question of fact. It may be an explanation, but the facts remain.

Thus man in these characters resembles much more closely the lower Cercopithecidae and Cebidae than his supposed

nearest congeners, at present existing. It is also incorrect to assert that only in man, a few American monkeys, and the anthropoid apes, does the hair slope towards the elbow. This Human Type is seen in the corresponding area of this segment of the anterior extremity of almost all hairy mammals, excepting most of the Ungulate types, and those with woolly hair. It is found very constantly in Carnivores, especially those which frequently rest in a "couchant" attitude, in which the head is held erect, the fore-limbs planted in front of the body, and the extensor surface of this limb-segment resting flat on the ground, also in certain other positions of rest; and it can be seen in nearly all wild Carnivores and domestic cats and dogs. In those Carnivores which assume this attitude the posterior limbs adopt a much more variable "pose," and here there is no constant form of hair-slope. The backward curl of hair on this narrow area of the fore-arm in man, certain monkeys, and many other hairy mammals, seems to be due to a mechanical force, slowly acting downwards and forwards, which makes for this direction of hair-slope. In all these three classes it is obvious that such pressure is frequent. This explanation of an inherited character, maintained by a simple physical cause, meets the case far better, I submit, than any supposed tracing out of ancestral vestiges.

WALTER KIDD.

### IS ANIMAL LIFE POSSIBLE IN THE ABSENCE OF BACTERIA?

SOME ten years ago Pasteur, in one of those "causeries du laboratoire" which those who were privileged to take part in will never forget, discussed with the young scientific men around him the interest which would attach to the nourishment of an animal from its earliest existence with sterilised food under conditions which would ensure the absence of all microbial life. "Sans vouloir rien affirmer," he added, "je ne cache pas que j'entreprendrais cette étude, si j'en avais le temps, avec la pensée préconçue que la vie dans ses conditions deviendrait impossible. . . . Que le résultat soit positif et confirme la vue préconçue que je mets en avant ou qu'il soit négatif et même en sens inverse, c'est-à-dire que la vie soit plus facile et plus active, il y aurait un grand intérêt à tenter l'expérience."

To decide this question Messrs. George Nuttall and H. Thierfelder have carried out elaborate experiments in the Hygienic Institute of the Berlin University with young guinea-pigs removed from the mother by means of the Cæsarean operation. Every conceivable precaution was taken to prevent all access of bacterial life. The young guinea-pig was placed in a sterilised chamber, supplied with sterilised air, and it was fed exclusively upon sterilised milk. It had to be supplied with food every hour, day and night, a process which so exhausted the investigators that at the end of eight days, when it had consumed 330 cubic centimetres of milk, and to all appearances was in perfect health and spirits, it was killed.

A microscopic examination of the contents of the alimentary canal revealed no bacteria whatever; aerobic and anaerobic cultures in various media were further made of the intestinal contents and of the excreta, but in every case the culture tubes remained sterile, not a single colony made its appearance. Messrs. Nuttall and Thierfelder claim by these experiments to have proved conclusively that the presence of bacteria in the alimentary canal is not essential to vital processes, at any rate in the case of guinea-pigs; and they consider themselves justified in assuming that other animals, and also human beings, could similarly exist in the absence of bacterial life, as long as the food supplied is purely animal in character. Whether the conditions would be altered by the addition of *vegetable* food to the diet, they next endeavoured to determine. In this series of experiments the food selected was so-called "English" biscuits containing about 7 per cent. nitrogenous material, 9 per cent. fat, 17 per cent. sugar, 58 per cent. of other non-nitrogenous matters, and 0.2 per cent. cellulose; these, together with the milk employed, were sterilised before use. The same rigorous precautions characterised these experiments as the previous ones; more animals were, however, secured, and they were allowed to live longer. The weight of the animals was this time carefully noted, and during the ten days, during which the experiment lasted, one animal gained 23 grammes and another 11 grammes. This calculation could only be an approximate one, as the experimental animals were not weighed when originally removed from the mother, and their initial weight was

only arrived at by weighing the other guinea-pigs which were removed at the same time, but not experimented upon. Thus in the case of vegetable substances bacterial life is apparently also not essential for carrying on digestive processes. The authors made also as careful an examination as was possible with the limited amount of material at their disposal, of the urine, and state that aromatic oxyacids were undoubtedly present. This result they regard as confirmatory of E. Baumann's assertion that aromatic oxyacids may be elaborated independently of intestinal decomposition. To this point they intend, however, to return later; at present further investigations are in progress with fowls, and the results will be awaited with the greatest interest, while immense credit is due to the authors for the ingenuity of the methods they have devised, and the self-sacrificing laboriousness with which they have conducted the experiments.

### SOCIETIES AND ACADEMIES.

#### EDINBURGH.

**Royal Society, Dec. 21, 1896.**—Lord Kelvin in the chair.—The first paper, on atomic configurations in molecules of gases according to Boscovich, was by the President himself. At the outset Lord Kelvin confessed that the problem was quite beyond him, and he only desired to throw out some suggestions. Boscovich's theory would quite well explain the atomic configuration of a gas if we could only apply it. In a monatomic gas the problem was fairly easy, collision between molecules leading to change in direction, either backwards on the original path, or at an angle, according as the impact was direct or oblique. For a diatomic gas we must imagine a "pair of something" held together by a mutual force which knocked about like one. He thought he could see why a diatomic gas should become monatomic when its temperature was sufficiently raised. But he could not yet understand why, when the process was reversed, molecules should combine in quartettes rather than in pairs, or triplets, and he illustrated his conjectures by means of models. He showed by means of these how, for example, the mutual repulsion between the H's might prevent O from combining with any more than two, and hence we did not have H<sub>2</sub>O. And he explained, similarly, how O<sub>3</sub> was unstable, as the octohedral arrangement of the atoms (taking O = O<sub>2</sub>) was easily broken up. But the whole subject was one of tremendous difficulty.—In an abstract from a paper on the cæcal fossæ, Dr. Richard Berry pointed out that the pericæcal folds and the resulting fossæ were primary in origin, and vascular in evolution. He strongly dissented from Treves' view that the meso-appendix is a substituted mesentery, maintaining that the ilio-colic and ilio-cæcal folds were the true cæcal mesenteries, primary and subsidiary respectively, the meso-appendix being the true appendicular mesentery. Arguing from this and other facts which he adduced, Dr. Berry stated that it would almost appear as though the appendix were gradually replacing the cæcum in functional activity. Passing on to the retro-cæcal fossæ, he pointed out the inaccuracy of the term retro-cæcal as applied to these fossæ, suggesting for them the name retro-colic as being more accurate and more scientific. He proceeded to show that these fossæ were secondary in origin and depended for that origin upon the secondary coalescence, sometimes wanting, of the colon, cæcum, and mesentery, to the posterior abdominal wall. In this respect Dr. Berry differed from almost every British author. He pointed out the variability of these fossæ in number and position, and strongly emphasised their importance to the surgeon in view of the prevalence of appendicitis and the part which these fossæ, according to the author, play in the etiology of that disease.—Dr. T. H. Milroy read a paper dealing with research into the nature of the nucleins and paranucleins of the animal cell. During the last few years much attention has been paid to two great classes of proteids intimately connected with the life of the cell, viz. the nucleins and paranucleins. The former class has been rather vaguely defined as including proteids which have only two points in common—a high percentage of phosphorus in organic combination, and a marked resistance to the action of the gastric secretion. The natural nucleins examined were those of the thymus gland of calves, of the red blood-corpuscles of birds, and of the pancreas of the ox; and these were found to agree in almost every particular with artificial syntonin-nuclein. That is, they were only slowly dissolved, not decomposed by the gastric juice (with the exception of the pancreas nuclein), while trypsin and sodium