are due to the zeal and ability of my colleagues. Thus, Mr. R. L. Jack has the merit of detecting and tracing the Caradoc basin of the Leadhills, and of working out the structure of that region which has been of so much service in the subsequent progress of the Survey. Mr. John Horne has carried the lines far into Galloway, and Mr. D. R. Irvine has traced them across a great part of Wigtownshire. Mr. H. Skae has mapped them across Dumfriesshire into Selkirkshire, while Mr. B. N. Peach, besides doing excellent service in the west, is now running them across the rest of the country towards the sea on the east.

Allow me also on the part of my colleagues, as well as for myself, to take advantage of this opportunity to thank Prof. Harkness for his most valuable and welcome papers, and to express our gratification that the labours of the Survey should have found so courteous an exponent, and one whose knowledge of the country which we have mapped is so minute and extensive.

ARCH. GEIKIE

The Huemul

IN Vol. viii. p. 253 of your valuable journal, I find it noticed that the Chilian Exploring Expedition has taken a specimen of the Huemul, an animal which had altogether been lost sight of, and first described by Molina under the name of *Equus bisulcus*. This notice is not correct, as the animal has been described already in 1846 by Messrs. Gray and Gervais, in the *Annales des sciences natur.* iii. Ser. Tom. v. page 91, under the name of *Cervus chilensis*, and compared with *C. antisiensis* of D'Orbigny, as the species most nearly allied to the Huemul or Guemul or Guamel, different names for the same animal in different parts of the country. This first description was repeated the following year in the "Historia fisica y Politica de Chile," Zoology, vol. i. page 159, and accompanied by the figure of the animal (pl. 10, and its skull pl. 11), from the only known specimen of a young male of half-grown size, brought to Paris by Mr. Gay. On the same specimen Mr. Pucheran has founded his description in his valuable monograph of the genus *Cervus*, published in the *Archives du Museum*, vol. vi. page 965 (1862), and from these two descriptions Mr. A. Warner has given a combined extract in his "Saugethiere," &c. Tom. v. (supplement), page 382, under the same name of *Cervus chilensis*. Meanwhile Dr. J. E. Gray had described a species of deer, received by the Earl of Derby from Chili as *Cervus leucotis* (Annals of Nat. Hist. ii., Jer. Tom. v. page 324, 1840, and Proceed. Zool. Soc., 1849, page 64, pl. 12), which name he soon changed to *Furcifer huamel* (Annals chr. iv. 427), and at last to *Xenelaphus huamel*, adding to his first description new notices, with the figures of the horns of the male (Proceed. Zool. Soc., 1869, page 496), and the skull of the female, and stating that his *Cervus leucotis* is identical with the *Cervus chilensis* of Gay. This exposition proves that the Huemul or Guemul is already a very well known animal, and has by no means been overlooked by naturalists.

A young collector in Buenos Ayres, Mr. Franc Moreno, has lately received a pair of these animals from Patagonia, where they were caught by the Indian Pehnelches, who live on the western foot of the Cordilleras, near the sources of the rivers Negro and Colorado. These two specimens have been brought to the public Museum, where I have examined them carefully. The male is a young one, with horns still covered by the skin, and only 3 in. long, without branches. I regret that therefore I can say nothing about the figure of the adult's horns, which are according to the drawing given by my dear friend, Dr. Gray, very like that of the roebuck, although the specimen he has figured may be regarded as in an abnormal state, from the great difference between their two sides. Both sexes of the animal are of equal size—3 ft. high on back, and 4½ ft. long, the head being 10 in. long, the ears and the neck 7 in. every one, and the body 3 ft. without the tail, which measures 7 in. with its hairs, but only 4 in. in the axis. Great naked lacrymal pits are seen below the eyes. The fur is of the same quality in each, but very different in the cold and in the warm seasons; then both skins are in the time of hairing, the female with the prevailing hair of the winter, and the male with the prevailing of the summer. Each hair is not entirely straight, but some are undulated, principally on the under half, and this undulated portion has a clear greyish-brown colour; over this clearer portion comes a broad dark-brown or black ring, and the end is clear reddish yellow, with a fine blackish tip, generally broken off in old fur. For the winter dress the hairs are 2 in. to 2½ in. long, and of a less characteristic colour, being over the whole skin of an undistinguished

greyish-brown colour; but in the summer dress the hairs measure no more than 1¼ in. or 1½ in., and all their colours are cleaner and better pronounced. Therefore the animal is darker and more distinguished in colour in the summer than in the winter. The hairs on the face are very short, as are those on the outside of the ears, somewhat longer on the legs, but nearly as short on the under half part of the extremities. The breast and the tail have the longest hairs. Different in colour are the naked nose and upper lip, both entirely black; the breast is dark blackish-brown, the genital region to the tail, with the inside of the hinder upper legs being white. The same colour also pervades the inside of the ears, which are coated with long hairs; the hoofs are black. No tinge of the particular stripe of longer hairs on the tarsus of the hinder legs is conspicuous in either sex; but I find, with Dr. Gray, a large tuft of longer hairs on the hock, on the inside behind, which makes this part of the legs very thick.

The animal lives principally in the valleys of the Cordilleras, but on both sides, the eastern and the western, and rarely goes down to the flat country of the Argentine pampas. Its proper range is between 35° and 45°. It is well known by the Indians, who not only make use of its strong skin for ward-dress, and its meat for food, but also tame young animals, using them for domestic employment, like the Guanaco, which lives in the same territory, but is much more common, and therefore almost the only animal used for hunting by the same people.

Buenos Ayres, Sept. 20

DR. BURMEISTER

The Diverticulum of the Small Intestine considered as a Rudimentary Structure

I MUST claim the opportunity of reply to the article which appeared in your number of October 16 (vol. viii. p. 509), entitled "On the Appendix Vermiformis and the Evolution Hypothesis," which the writer offers as a commentary on my little paper at the recent meeting of the British Association, "On the Diverticulum of the Small Intestine considered as a Rudimentary Structure."

The writer seems to have been misled by newspaper reports. None of these were furnished by me, or submitted to me before publication, and in those which I saw after their publication both the anatomy and the argument were grossly and indeed absurdly blundered. This applies not only to my paper and remarks, but to the remarks made by those who spoke on my paper. It was, perhaps, too much to expect newspaper reporters not to get confused among scientific terms, and I may have erred in not having the usual abstract of my little paper ready to hand to the reporters.

Newspaper reports may be passed without notice, but I cannot allow an article in a scientific periodical to pass in which the writer uses such language as the following, with which the article in your columns concludes:—

"To quote the words of one of the greatest of our physiologists, it can only bring ignominy on the body of scientific workers if they are supposed to countenance such an argument as that of Prof. Struthers, which assumes that because one or two individuals have died from the impaction of cherry-stones in the appendix vermiformis, therefore there is no God!"

The "no God" was certainly not in my paper or in anything I have ever written or spoken, and the accusation is to me so offensive that I repudiate it with indignation. How anyone should suppose that the evolution hypothesis implies that there is "no God" I am at a loss to understand. I supposed it to be well understood that, on the contrary, that great hypothesis enables us to rise to higher conceptions, the only question being the mode of proceeding.

As to the scientific argument, it seems hopeless to attempt to unravel the confusion into which newspaper reports and my critic have brought it, except by re-stating my argument. But this is for the most part unnecessary after your publication of my abstract in the number following that in which the article of which I complain appeared. It cannot be absolutely proved that the appendix vermiformis is useless, though a survey of the facts in comparative anatomy and development leads to the inference that it is a rudimentary structure. But my paper was on the diverticulum, the appendix being referred to only collaterally, and more for the sake of clearing away the most unnecessary teleology with which it has been encrusted, than with the view of resting the argument on it. The diverticulum, like the appendix, has glands and muscular layers, secreting and expelling; it has villi, actively absorbing; and it is large, which the appendix is not. Yet, notwithstanding all this elaborate construction and this activity, who will maintain that this unclosed bit of the vitelline duct has been left

unclosed in some of us for use? But one is sometimes met with the remark that, if these rudimentary and variable structures are useless, they are at any rate not injurious. But is it so? May they, and do they, not become injurious under disease or accident? There is the male mamma, for instance, which we have sometimes occasion to excise for disease. Whatever may be the law which regulates the evolution of the sexual organs, no "use" theory can account for the presence of that rudimentary organ. But the diverticulum is a possibly injurious structure not merely as a tissue, but in addition, specially, as forming, if I may use the word, a kind of trap, by lodgment or by strangulation. Thus we find that we have, whether we will or no, reached the conclusion that there are parts in the animal body which are not only useless but worse than useless because dangerous.

I do not see any reply to this in my critic's remark that it proves too much for the argument, that, for instance, because some people have died from wounds of the scalp, therefore the head might be dispensed with. For, however much the head may vary among us, it is not a rudimentary structure. No argument can affect the fact that the diverticulum is not only a useless structure, but worse than useless because dangerous. The object of putting it thus emphatically is both to establish and to call attention to the conclusion that there are such things in animal bodies as rudimentary structures, things which are of no use to the animal body which contains them, and which can be understood only by referring to other animal bodies in which they are in full play; and that we must therefore rise to higher conceptions of the mode in which these things are regulated. It was carefully stated in my paper that the consideration of such parts as the diverticulum does not carry us further than to clear away the old argument, but that, on taking a survey of rudimentary structures generally, we are led on to the conclusion that the evolution hypothesis is the more probable one in regard to the mode of origin of animal bodies.

The nature of the diverticulum and its sources of danger are well known to the readers of Meckel, Monro, Lawrence, Rokitsanski, and Cruveilhier. I may be allowed to mention that nearly twenty years ago I published (*Edin. Med. and Surg. Journal*, April 1854) twenty cases of diverticulum, with a drawing of each. In three of these it was the cause of death, and I referred to some other cases in which it caused death as reported by previous writers. Anyone in London who is desirous of seeing a case in which it caused death, may do so by looking into the museum of St. Bartholomew's Hospital. There is, I may mention, a diverticulum, at the usual place, in a subject now being dissected in my anatomical rooms. If my critic will come to Aberdeen I will show him a large collection of them, and also of specimens showing the various positions and conditions of the appendix vermiformis, and, indeed, many other interesting rudimentary structures and variations which, I infer, he has not yet seen.

My critic's objection that such discussions are unnecessary, that the true theory will ultimately prevail from its own intrinsic value, might be urged against all discussion; and I differ from him very much if he thinks that the question does not require to be stirred among and by the teachers of human anatomy in this country. The cause of my little paper, in fact, was my having, not long before, heard a teacher of human anatomy, at a similar meeting, call in question the whole argument from rudimentary structures. I attributed no importance to my paper further than that, in bringing forward the diverticulum, it submitted an illustration for the argument which does not admit of cavil.

Aberdeen, Nov. 22

JOHN STRUTHERS

The Atmospheric Telegraph

Will you permit one of your subscribers who is interested in the credit of the English telegraphic system, to supplement your article of November 27 by a few remarks?

The distribution of telegraphic messages by means of air was introduced by Mr. Latimer Clark, and had been employed by the Electric Telegraph Company long before it was adopted either in Berlin or Paris.

The *Times* article of November 15 deals with the undertaking of the Pneumatic Despatch Company for the conveyance of parcels and goods, not messages. The writer incidentally mentions the transmission of messages, but scarcely seems to have been aware of the extent of the London message system.

If I might encumber your valuable space by statistics, I could show that the pneumatic system of the Postal Telegraphs, or even that of the Electric Telegraph Company at the time of the transfer of their undertaking to the State, will bear comparison,

both as to extent and efficiency, with that of Paris, effective as the latter is.

The system is employed in Manchester, Liverpool, Birmingham, Glasgow, and Dublin.

R. S. CULLEY

Engineer-in-Chief of Postal Telegraphs
General Post Office, Nov. 29

SENSATION IN THE SPINAL CORD

THE principle which I endeavoured some years ago to get recognised as the directive principle of research in Nerve Physiology, was that everywhere identity of Tissue carried with it identity of physiological Property, and that similarity in the structure and connections of Organs involved corresponding similarity in Function. Although these premisses were almost truisms, the conclusion drawn—that all nerve-centres must have a *common* Property, and *similar* Functions—was too much opposed to the reigning doctrine, to find general acceptance. Especially was it resisted in its application to the functions of the Spinal Cord; and this because of the two hypotheses current, namely, that Reflex Action did not involve Sensibility, and that the Brain was the sole Organ of the Mind. Following in the track so victoriously opened by Pflüger, I brought forward what seemed to me decisive evidence of the sensational and volitional functions of the Spinal Cord; but this evidence has not been generally deemed conclusive by those whose verdict is authoritative. Neither in Germany nor in England have the majority of physiologists consented to regard the actions determined by the Spinal Cord in the absence of the Brain as sensitive actions.

This is not the place to examine the insufficiency of the evidence which is held to exclude sensation from Reflex Action, nor to exhibit the irrationality of the conception of the Brain as the Organ of Mind—which, as I have elsewhere said, is not more acceptable than would be the parallel conception of the Heart as the Organ of Life. The purpose of the present paper is restricted to the examination of the most striking experimental evidence *against* the sensational function of the Spinal Cord, which to my knowledge has hitherto been advanced. I had intended reserving the criticism for its appropriate place in the "Problems of Life and Mind," but an article by Mr. Michael Foster which has just appeared (*Journal of Anatomy and Physiology*, November), on the Effects of rise of Temperature on Reflex Action, induces me to bring the subject before the readers of *NATURE*, in the hope that some of them may re-investigate it and record their results.

I will merely remark that the microscopic investigations which have recently been made with greatly increased powers and better methods of preparation, have more and more confirmed my assertion of the histological identity of Spinal Cord and Brain. On the other hand the experiments of Goltz (*Funktionen der Nervencentren des Frosches*, 1869, p. 128) seem to supply direct evidence against the identity of property; and this evidence cannot be ignored.

Goltz observed that a frog, when placed in water the temperature of which is slowly raised towards boiling, manifests uneasiness as soon as the temperature reaches 25° C., and becomes more and more agitated as the heat increases, vainly struggling to get out, and finally, at 42° C., dies in a state of rigid tetanus. The evidence of feeling being thus manifested when the frog has its brain, what is the case with a brainless frog? It is absolutely the reverse. Quietly the animal sits through all the successions of temperature, never once manifesting uneasiness or pain, never once attempting to escape the impending death. "The spinal soul sleeps, perhaps; it takes no heed of the danger. One must waken it. I touch with acid the skin of its back in that part which is raised above the surface of the water. *Swiftly and surely the hind paw is brought to bear on it, and the acid on the irritated spot is wiped away*; then the leg resumes its comfortable position."