than ordinary interest. To one of these — his conclusion that Cortez, in his expedition to Honduras, visited Palenque, and found it then inhabited — I call the special attention of the readers of *Science*.

This conclusion is based chiefly upon his study of Cortez' route in his journey southward. He identifies as Palenque the town which Herrera names Titacat, and which, according to Bernal Diaz, was the first reached after the execution of Cuauhtemoctsin, and where Cortez, unable to rest at night, "went into a large apartment where some of the idols were worshipped," missed his way, and fell some 'twelve feet,' receiving a severe wound in the head, and in reference to which Cortez writes as follows: —

"It is a very beautiful village: it is called Teoticcac, and has fine temples, especially two, in which we are lodged, and from which we have cast out the idols, for which they do not show much regret; for I had already spoken to them of it, and had shown them the error in which they rested, and that there was but one God, creator of all things. . . . I learned of them that one of these two houses, or temples, which was the most important, was sacred to a goddess in whom they placed much confidence and hope, and that they sacrificed to her only young and beautiful maidens. If they were not such, then she would be very angry with them; and for this reason they always took great care to seek them, that she might be satisfied; and they brought up from infancy those who were of good appearance to serve this purpose."

Our author comments on this letter as follows: —

"This description by Cortez applies perfectly to Palenque. There are, indeed, at this place, besides numerous temples and buildings, two principal edifices. One contains the great hall of mural inscriptions: the other is the convent of the virgin priestesses, which has been wrongly taken until now for the palace of the king."

Is this conclusion justifiable? It has generally been admitted that the route followed must have brought the Spanish conqueror within a few miles of this place: hence the opinion advanced cannot be considered as doing violence to the history of the expedition in this respect. If inhabited at that time, it is not probable that he would have approached within twenty-five or thirty miles without visiting it, as it must have been, during occupancy, a place of considerable notoriety and importance.

Stephens was led by his examinations to believe the ruins of Yucatan were inhabited villages and cities down to a comparatively modern date, some of them being occupied until the conquest by the Spaniards. Charney's explorations led him to the same belief. He remarks in one of his letters published in the *North-American review*, —

"It is certain, that, at the time of the conquest, the coast of Yucatan and Tabasco was covered with towns, pyramids, and monuments, all of which were inhabited. And if such were the case with the coast, what is the inference that must be drawn as to the interior? . . . If the palaces of Comalcalco were entire and inhabited at the time of the conquest, we may feel bound to conclude those of Palanque were in the same condition. . . . Altogether, it seems to be sufficiently established that these monuments were inhabited at the date of the conquest, and that they are the productions of a comparatively modern era."

And now Maler, who has gone carefully over the ground in person, and studied the country and the ruins for himself and in his own way, comes to precisely the same conclusion. We are therefore convinced that there is nothing in the age of the ruins to forbid the idea that Cortez visited the place, and found it inhabited.

It is also worthy of notice that Charney agrees with Maler in considering Palenque a 'holy place,' a 'religious centre,' and that the so-called 'palace' must have been 'the home of priests, and not of kings.'

Our author's theory will afford at least a partial explanation of some of the figures found on these ruins; as, for example, the frequent representations of children in the arms of males and females, the repeated occurrence of female figures, and the fact, as shown in Stephens's plates, that the heads of most of these are obliterated, which I have long suspected was due to the fanatical zeal of Catholic priests, who visited the place at an early day. Cortez' visit will furnish a complete explanation of this fact, which does not appear to have attracted the attention its importance demands.

Cyrus Thomas.

DO ANIMALS EXCRETE FREE NITROGEN?

Many of the older experiments upon the nutrition of animals included determinations of the nitrogen of the food and of the visible (solid and liquid) excreta. Almost invariably the latter quantity was notably less than the former, and as a consequence it was commonly held that the difference was excreted in gaseous form through the lungs. In process of time, however, as the methods of experiment were refined, this deficit began to diminish in amount, until now it is indisputably shown that the great difference found by the earlier experimenters was very largely due to mechanical losses of the excreta. A certain insoluble residue, however, still remains, which has been the occasion of not a little contro-

versy among physiologists; one school maintaining, and another denying, that it is to be interpreted as showing an excretion of gaseous nitrogen.

There is one fact which renders the results obtained by the experimental method just mentioned inconclusive either for or against an excretion of free nitrogen: it is that the animal experimented upon may either gain or lose nitrogenous matter from the tissues of its body during the experiment. If the former take place, the excretion of nitrogen is diminished by that amount: if the latter happen, it is increased. But, while such gain or loss of nitrogenous matter by the body may undoubtedly take place, we have no means of proving that a small gain or loss has or has not occurred in any given experiment. If in some trial the nitrogen of the excreta exactly equal that of the food, the advocate of the excretion of gaseous nitrogen can say that a certain (unknown) amount of nitrogen *may* have been lost from the body of the animal, and, by chance, the same amount *may* have been excreted as gas. If an experiment show a deficiency of nitrogen in the excreta, the denier of the excretion of free nitrogen can say that exactly that amount of nitrogen *may* have been gained by the animal. Plainly, neither of these possibilities can be either proved or disproved by this method of experiment.

A resort to an investigation of the respiratory products naturally suggests itself. The experiment, though a difficult one, has been made; but the results have not, as might have been hoped, sufficed to decide the question definitely.

It should be remembered that the amount of nitrogen excreted as gas must, in any case, be small. The large deficit found by the earlier experimenters is universally acknowledged to have been erroneous. Bearing this in mind, it is evident, that, as already pointed out, a single experiment by the first method has comparatively little weight. But very many such experiments have been made, and, when properly made (i.e., on mature animals, with food just sufficient to maintain them without gain or loss of weight), they all agree in showing a very small difference between the nitrogen of the food and that of the excreta; and, moreover, the difference is sometimes in one direction, and sometimes in the other. For example: out of forty-three experiments by various observers, whose results chance to lie before me, nineteen show an excess of nitrogen in the excreta, and twenty-four a deficiency, as compared with the nitrogen of the food. The excess varies from 0.07% to 6% of the total nitrogen fed; and the deficiency from 0.02% to 6.7%. Many more observations might be quoted to the same effect. Such results as these have a cumulative force, and go far to establish the hypothesis that there is no excretion of gaseous nitrogen.

Some of the believers in an excretion of gaseous nitrogen, particularly Seegen and Norwak in Vienna, have attacked these results upon the side of the analytical methods employed, claiming that the process (soda-lime process) used for estimating nitrogen gives too low results. It has been shown, however, by several chemists, that this is not the case when the process is properly performed; while some recent trials by Gruber[1] show, that, when the so-called 'absolute method' for nitrogen is employed, substantially the same results are reached.

The main reliance of those who believe that animals excrete free nitrogen, however, is upon respiration experiments, nearly all of which appear to favor their view. These experiments are made substantially in the following manner. The animal breathes in a confined volume of air of known amount, whose exact composition is determined by analysis before the experiment begins. As the oxygen of the confined air becomes exhausted, measured quantities of pure oxygen are admitted from a gas-holder, while the carbonic acid which is exhaled is absorbed by caustic potash. At the close of the experiment the air in the apparatus is again analyzed; and the observer then proceeds to compute, from the data he has secured, the amount of nitrogen originally present in the air within the apparatus, and the amount remaining at the close of the experiment. If the latter quantity is found to be the larger, it shows (barring experimental errors) that the animal has exhaled gaseous nitrogen.

Almost, if not quite, every experiment made on this plan has shown an apparent small excretion of free nitrogen. Thus the well-known experiments of Regnault and Reiset appear to show an excretion of free nitrogen by various animals. In their experiments with small animals the amount was relatively small; and sometimes an absorption of nitrogen was observed, especially during hunger. In experiments with larger animals (sheep and calves), in a larger apparatus, the apparent excretion was quite considerable.

Seegen and Norwak in Vienna have reported numerous trials with a simplified form of Regnault and Reiset's apparatus, all of which show an apparent excretion of nitrogen; and a lively debate has been carried on between them and Voit, each party endeavoring to explain away the results of the other.

Some recent experiments by Leo[2] are of much interest in this connection. He worked with rabbits, which were tracheotomized and supplied with pure oxygen. After sufficient time had elapsed to remove all free nitrogen from the lungs, the expired gas was collected, and found to contain nitrogen corresponding to an excretion of over 8 mgr. per hour and kilogram of body-weight. This result was obtained when the animals were located in free air. In a second series the head of the animal was cemented into the apparatus. The excretion sank to 2–3 mgr. per hour and kilogram. Finally, in a third series, the whole body of the animal was immersed in a warm bath in order to hinder possible diffusion of atmospheric nitrogen into its cavities, and the excretion was reduced to 0.3–0.5 mgr. per hour and kilogram, or to about one-twelfth the amount found by Seegen and Norwak.

It thus appears that the greater the care taken to

[1] *Zeitschr. für biologie*, xvi. 367.
[2] *Jahresber. thier. chem.*, xi. 382.

exclude atmospheric nitrogen from the apparatus employed, the less becomes the apparent excretion of nitrogen by the animal. This, taken in connection with the similar fact already mentioned, regarding the results of experiments by the other method, is significant. If, as we increase the delicacy of our experimental methods, the apparent excretion of free nitrogen becomes less and less, it is not a very bold assumption which regards it as entirely due to the unavoidable errors of experiment. That such is the case is perhaps not proven, but the weight of evidence is decidedly in favor of that belief.

H. P. Armsby.

THE BRITISH NAUTICAL ALMANAC.

We have received promptly, as usual, the "Nautical almanac and astronomical ephemeris for the year 1888, for the meridian of the Royal observatory at Greenwich," the contents and arrangement of which are announced to be the same generally as those of the preceding year. We find no changes in the adopted astronomical constants, nor have any new prediction-tables been substituted for those which have now been employed for many years. The early Struve constant of aberration is not replaced by the recent Pulkowa determination, and Newcomb's mean equatorial horizontal parallax of the sun, 8.848″, is wisely retained. The fundamental elements of the moon's position in space are derived from Hansen's tables unaltered, and the apparent positions only are modified by Newcomb's corrections, — a method of procedure which seems to be best adapted to the needs of the future investigator.

For the first time in the history of nautical almanacs, the positions of all the great planets were derived from a uniform system of tables, and so published in the British 'Nautical almanac' for 1882; and the use of these same tables is still adhered to. These are the planetary tables constructed by the late Leverrier, and printed in the fifth, sixth, twelfth, and fourteenth volumes of the 'Annales de l'Observatoire impérial de Paris.' The derivation of the times of the phenomena of Jupiter's satellites is based on the 'Tables écliptiques des satellites de Jupiter, par le Baron de Damoiseau,' Paris, 1836. Professor Adams's extension of these tables, now employed in the British 'Nautical almanac,' will expire in two years more.

This ephemeris is now most deficient in its list of standard stars, the number and relative positions of those in the list being entirely inadequate to the needs of field and observatory work. Catalogues of stellar co-ordinates of high precision are now so numerous that there would seem to be no good reason why the British 'Nautical almanac' should hesitate in following the 'Berliner astronomisches jahrbuch,' the 'Connaissance des temps,' and the 'American ephemeris,' all of which have within a few years adopted very full lists of standard stars. Also great improvements might be suggested for other parts of the work.

Ever since the year 1834, when the English 'Nautical almanac' became an astronomical ephemeris as well, the management of this publication has been characterized by a conservatism, which, in these times of change just for change, is delightful to behold. But even conservatism may be unwise; and, if the British 'Nautical almanac,' as an astronomical ephemeris, is to hold in the future the place it has held in the past, a committee of reconstruction, somewhat like that 'relative to the improvement of the Nautical almanac' in 1830, would seem to be required to effect the needed modifications.

David P. Todd.

CONTAGIOUS DISEASES OF DOMESTIC ANIMALS.

The agricultural department at Washington has just issued a volume of some three hundred and fifty pages devoted to the above subject, as the result of the investigations of its veterinary division, — an office distinct from the more newly established 'bureau of animal industries.' The subject-matter, being made up of the reports of the veterinarian-in-chief and his assistants, is of a sort that will, in a way, be interesting and instructive reading for veterinarians, and to a certain extent for comparative pathologists.

The volume opens with a description of a 'veterinary experimental station' recently located, in connection with the department, near Washington, which seems to afford abundant facility for the proposed work, and from which, in the future, much that will tend greatly to aid in protecting our animal interests from the ravages of disease will undoubtedly result. Then follows a detailed report of outbreaks of contagious pleuro-pneumonia among cattle in Connecticut, New Jersey, Pennsylvania, and Maryland. These have an historical interest, but nothing more, because these states have repeatedly been shown to contain this exotic disease; and it has just as repeatedly been shown that a more or less constant interchange of it goes on with the natural traffic of cattle within their borders.

An exceedingly interesting and carefully written report is made by Dr. Salmon upon an enzoötic outbreak of ergotism among cattle in Coffey county, Kan. It is very much to be regretted, for the sake of the department, the cattle interests of Kansas, and the veterinary profession, that, under the circumstances, Dr. Salmon did not himself attend to the matter when first it was reported to be an outbreak of 'foot and mouth disease,' instead of trusting so important a decision to such an unsafe man as 'V. S.' Trumbower proved to be, who, by his own report of the matter given in this same volume, seems to have arrived upon the ground on the afternoon of March 8, to have examined the cattle and their surroundings carefully, and to have then entertained the opinion that the trouble was due to 'foot and mouth disease,' until the 20th of the same month, when he was joined by Dr. Salmon. He then suddenly became as firmly convinced that the trouble was due to ergotism. Is