

- III. "On the Placentation of the Lemurs." By WM. TURNER, M.B. (Lond.), Professor of Anatomy, University of Edinburgh. Communicated by Prof. HUXLEY, Sec. R.S. Received February 21, 1876.

(Abstract.)

In the Introduction to this Memoir a description was given of the observations made by M. Alphonse Milne-Edwards on the gravid uteri of several genera of Lemurs. The author then proceeded to describe the gravid uteri of six Lemurs which he had received from Dr. Andrew Davidson, of Antananarivo, Madagascar, viz. *Propithecus diadema*, *Lemur rufipes*, and *Indris brevicaudatus*. He then summarized the conclusions he had arrived at in the course of his dissections, and showed that the placenta in these animals was diffused and presumably therefore non-deciduate. The paper concluded with a discussion of the bearing of these observations on the classification of the Lemurs, and on the theory propounded by Hæckel of the descent of the deciduate mammals from a primæval root-form of Prosimiæ.

- IV. "On the Movement of the Glass Case of a Radiometer." By WILLIAM CROOKES, F.R.S. &c. Received March 30, 1876.

During the discussion which followed the reading of Prof. Reynolds's and Dr. Schuster's papers at the last meeting of the Royal Society I mentioned an experiment bearing on the observations of Dr. Schuster. I have since tried this in a modified form; and as the results are very decided and appear calculated to throw light on many disputed points in the theory of these obscure actions, I venture to bring a description of the experiment, and to show the apparatus at work, before the Society.

I made use of a radiometer described in a paper communicated to the Society in January last. I quote the description from paragraph 184. "A large radiometer in a 4-inch bulb was made with ten arms, eight of them being of brass and the other two being a long watch-spring magnet. The disks were of pith, blackened on one side. The power of the earth on the magnet is too great to allow the arms to be set in rotation unless a candle is brought near; but once started it will continue to revolve with the light some distance off."

This radiometer was floated in a vessel of water; and four candles were placed round it so as to set the arms in rotation. A mark was put on the glass envelope so as to enable a slight movement of rotation to be seen. The envelope turned very slowly a few degrees in one direction, then stopped and turned a few degrees the opposite way; finally it took up a uniform but excessively slow movement in the direction of the arms, but so slow that more than an hour would be occupied in one revolution.

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A powerful magnet was now brought near the moving arms. They immediately stopped, and at the same time the glass envelope commenced to revolve in the opposite direction to that in which the arms had been revolving. The movement kept up as long as the candles were burning, and the speed was one revolution in two minutes.

The magnet was removed, the arms obeyed the force of radiation from the candles and revolved rapidly, whilst the glass envelope quickly came to rest and then rotated very slowly the same way as the arms went.

The candles were blown out; and as soon as the whole instrument had come to rest a bar-magnet was moved alternately from one side to the other of the radiometer, so as to cause the vanes to rotate as if they had been under the influence of a candle. The glass envelope moved with some rapidity (about one revolution in three minutes) in the direction the arms were moving. On reversing the direction of movement of the arms the glass envelope changed direction also.

These experiments show that the internal friction, either of the steel point on the glass socket, of the vanes against the residual air, or of both these causes combined, is considerable. Moving the vanes round by the exterior magnet carries the whole envelope round in opposition to the friction of the water against the glass.

As there is much discussion at present respecting the cause of these movements, and as some misunderstanding seems to prevail as to my own views on the theory of the repulsion resulting from radiation, I wish to take this opportunity of removing the impression that I hold opinions which are in antagonism to some strongly urged explanations of these actions. I have on five or six occasions specially stated that I wish to keep free from theories. During my four years' work on this subject I have accumulated a large fund of experimental observations, and these often enable me to see difficulties which could not be expected to occur to an investigator who has had but a limited experience with the working of one or two instruments.

Presents received, March 2, 1876.

Transactions.

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