something like 30 tons per square inch would be required. If the natural weight of the cable is nearly 400 times as great as the counter-force on it produced "centrifugally," we could imitate this outwards, by supposing the earth to revolve about 20 times faster than it is actually doing; and then the end pulls required to make a telegraph cable relieved from gravitation and acted on only by this outward "centrifugal force," just equal to the cable's weight, to stretch it to as flat a curve as that of the earth's circumference and of its circles of latitude, would be about 20×30 tons per square inch, or vastly more, of course, than any cable could withstand! But what telegraph-engineer would ever dream of trying to stretch telegraph wires on poles along the banks of a canal so that even between poles they should everywhere be absolutely parallel to the water level surface in the canal? With their natural weight acting outwards against him he would know that the attempt would be sure to result in rupture! And even if the centrifugal force due to the earth's velocity in this latitude of about 1250 feet a second acted alone on the wires instead of the above supposed one of about 5 miles a second to represent an equivalent to gravity, he would surely not be surprised to find his attempt to pull the wires as straight as the surface of still water, end only unsuccessfully in breaking them?

A. S. HERSCHEL.

New Athenæum Club, London, March 20.

Spinning Disks.

MR. WEHAGE, of Berlin, has independently obtained Prof. Ewing's results for a solid disk by putting $p_1 = p_2$ for $r = 0$, in Grossmann's equations.

As Prof. Boys points out, Maxwell attacked the problem in 1850, at the age of 19. I have gone over his solution ("Papers," vol. i. p. 61), and find that, in consequence of two slips he has made, his results do not agree with Ewing's. His first equation of (57) should read $\frac{c_1}{2r^2}$, instead of $\frac{c_1}{r^2}$; and his third equation

of (57) will then read $-\frac{c_1}{2r^2}$, vice $-\frac{c_1}{r^2}$. His final result (59) is not affected by this alteration, which only changes c_1 and not c_2 ; but, owing to a wrong sign, that equation should read

$$q = h_1 + \frac{\pi^2 k}{2g^2} \left[-2(r^2 + a_1^2) + \frac{E}{m} (3r^2 - a_1^2) \right];$$

which, remembering that Maxwell's E and m are E and $E/(1+\mu)$ respectively in Ewing's notation [or Thomson and Tait's $gnk/(3k+n)$ and $2n$, respectively], is seen to be the same as

$$p_1 = h_1 + \frac{\omega^2 p}{8} \left[(3+\mu)a^2 - (1+3\mu)r^2 \right];$$

except that Maxwell takes tensions negative.

Maxwell's equation (60) is correct; but is not deducible from his (59).

As Prof. Pearson remarks (Todhunter and Pearson's "History of Elasticity," vol. i. p. 827), Maxwell seems to have thought the disk would yield first at the rim.

Cambridge, March 23. J. T. NICOLSON.

P.S., March 28.—Since writing the above, I have read Prof. Pearson's letter (NATURE, March 26, p. 488), in which he says that "Grossmann's results, such as they are, flow at once from Hopkinson's corrected equations." They flow, however, also at once from Maxwell's equations, if these are corrected in the manner I have shown above. Prof. Pearson is not correct in supposing that Maxwell's and Grossmann's solutions lead to different results for the position on the disk of the maximum hoop-tension. Maxwell probably thought the disk would yield first at the rim from his putting $r = a_1$ in his final result; but his corrected results are identical with Grossmann's for a hollow, and with Ewing's for a solid disk.—J. T. N.

The Stresses in a Whirling Disk.

By an unfortunate accident my letter on this subject was printed (NATURE, March 19, p. 462) without correction in proof. There are several *errata*, due to slips in the manuscript.

In column I, p. 462, line 10 from bottom, the words "hoop" and "radial" are transposed. Read "the hoop tension p_1 and the radial tension p_2 ."

In column 2, equation (7) should read

$$\frac{u}{r} = \frac{C}{r^2} + C_1 - \frac{(1-\mu^2)\omega^2 p^2}{8E} \dots \dots (7)$$

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Further down, the expression for the maximum radial tension in a disk with a central hole should read

$$\text{Max. } p_2 = \frac{\omega^2 p}{8} (3+\mu)(a_1 - a_2)^2.$$

And in the special case when the hole is very small,

$$\text{Max. } p_1 = 2 \times \text{Max. } p_2 = \frac{\omega^2 p a_1^2 (3+\mu)}{4}.$$

Cambridge, March 20.

J. A. EWING.

The Poison Apparatus of the Heloderma.

OUR largest United States lizard, the *Heloderma suspectum*, is too well known to science to require any special description here. In the Proceedings of the Zoological Society of London, for April 1, 1890, the writer published quite an exhaustive memoir upon the entire morphology of this famous reptile, and among other parts of its anatomy reference was made to its poison apparatus. Fig. 4 of Plate xvi., of the contribution in question, showed a superficial dissection of the under side of the head of a large *Heloderma*, and upon the left side of the same the submaxillary gland is turned outwards, thus rendering it possible to be seen the four structures leading from (or to) this gland to separate and as many foramina opposite them, which exist upon the external aspect of the ramus of the lower jaw. Heretofore, these four structures have very generally been looked upon as the four poison ducts leading from this gland to the hollow space in the mandible, which latter in turn had its upper outlets in the minute foramina, found one each at the base of the teeth of this jaw. The poison was supposed to pass from the gland through these four ducts into the body of the ramus, then through the above-noted foramina, whence it was conducted into any wound the reptile might inflict with its teeth, along the grooves which have for many years been known to exist upon them. Without making any especial microscopical description, this is practically the view I supported in my memoir in the Proceedings of the Zoological Society, and I added that "Fischer found in his specimen that these ducts branched as they quit the gland; this was not the case in the reptile examined by me. Each duct passes obliquely upwards and inwards through the lower jaw, and its internal opening within the mouth is found at the base of the tooth it supplies, near the termination of the groove of the tooth" (p. 207). Dr. Fischer's paper was published in Hamburg in 1882, and in it he also gives a figure showing the ducts I have just mentioned; and he is largely responsible for the view that has been adopted in regard to them. As early as 1857, however, John Edward Gray evidently leaned in the same direction, and he speaks of "*Heloderma horrida*, in which all the teeth are uniformly furnished with a basal cavity and foramen,"—structures which he compared with the poison fangs and associated parts of venomous serpents. Dr. C. K. Hoffmann had the same ideas about the poison apparatus of the *Helodermas* (Bronn's "Klassen des Thier-Reichs," Bd. vi., iii. Abth., 30-32 Lief., pp. 890-892), and he republished Fischer's figures; and thus running through the similar views of numerous other leading herpetologists, we find, even as late as 1890, Prof. Samuel Garman, of Harvard University, quoting Fischer's description of the poison glands of *Heloderma* without question (author's reprint from Bull. Essex Inst., vol. xxii., Nos. 4-6, 1890, p. 9), and he adds that "no glands have been found on the upper" jaw. With all this in my mind, I was not a little surprised when Prof. C. Stewart, on January 20 last, in a paper read before the Zoological Society of London, claimed that he believed "that he had shown that in both species [*H. horridum* and *suspectum*] the ducts of the gland did not enter the lower jaw, but passed directly to openings situated under a fold of mucous membrane between the lip and the jaw. He thought that the structures previously described as ducts were only the branches of the inferior dental nerve and blood vessels." Upon hearing this, I at once took a large specimen of *Heloderma suspectum* to my friend Dr. E. M. Schaeffer, the well known microscopist in Washington, D.C., who kindly examined the structures, and found them to be exactly as Prof. Stewart had described. It now remains to be said of what use are the foramina at the base of the teeth in these lizards. Why are the upper teeth grooved when there is no poison gland upon the upper jaw? Would not such a severe bite as a *Heloderma* is enabled to give kill "frogs and insects," even were no poison injected into such wounds? Are the grooves on the teeth there to conduct a poison into the wounds

inflicted by the teeth of this lizard? Here in America the evidence would seem to be rapidly leading to the demonstration of the now entertained theory that the saliva of this heretofore much-dreaded reptile is possibly almost entirely innocuous. If it be proven, it is unnecessary to add that it leaves the great order *Lacertilia*, in so far as our present acquaintance goes with it, without a single representative possessed of the power of inflicting a venomous wound by the means of its bite.

R. W. SHUFELDT.

Smithsonian Institution, February 24.

The Recession of the Niagara Gorge.

BEFORE the survey of 1842, the only data for estimating the rate of recession of the Niagara Gorge were the observations of the people of the neighbourhood. Mr. Bakewell, in 1829, "was informed by Mr. Forsyth, the proprietor of the Pavilion Hotel on the Canada side, that during his residence of forty years the Falls had receded forty yards" (*American Journal of Science*, May 1857, p. 85). It is well-known that Sir Chas. Lyell, at the time of his visit to the Falls, came to the conclusion that the rate of recession was not over one foot a year. He based his estimate upon the statement of his guide, that between 1815 and 1841 the American Fall had receded forty feet. Modern investigators, basing their calculations on the surveys of 1842, 1875, and 1883, estimate the recession of the Canadian Fall at from three to five feet per year, and the age of the gorge from seven to ten thousand years (see Mr. Wesson's article in *NATURE*, vol. xxxii. p. 229; and Prof. Wright's "Ice Age in North America," p. 452 *seq.*). A recent American publication, "The Journal of William Maclay," a member of the Senate in the first Congress, 1789-91, brings to light the result of local observation of the recession of the Falls for the thirty years previous to the beginning of Forsyth's observations, which have generally been cited as the earliest. The passage in Maclay's journal reads:—"February 1st [1790]. . . . Mr. Ellicott's accounts of Niagara Falls are amazing indeed. I communicated to him my scheme of an attempt to account for the age of the world, or at least to fix the period when the water began to cut the ledge of rock over which it falls. The distance from the present pitch to where the Falls originally were, is now seven miles. For this space a tremendous channel is cut in a solid limestone rock, in all parts one hundred and fifty feet deep, but near two hundred and fifty at the mouth, or part where the attrition began. People who have known the place since Sir William Johnson took possession of it, about thirty years ago, give out that there is an attrition of twenty feet in that time. Now, if 20 feet = 30 years = 7 miles, or 36,960 feet; answer, 55,440 years." In view of the fact that since 1842 the rate of recession has varied widely, this earlier testimony, so far as it can be relied upon, is especially interesting. It is possible that the observation related to the American Falls, as did that of Sir Chas. Lyell's guide; but there is nothing to indicate it, except that this is about the present rate of recession of the American Fall as calculated by Mr. Wesson. The intelligent attitude and fertile scientific suggestion of Senator Maclay are very remarkable in 1790. It will be remembered that Hutton's "Theory of the Earth" was not published separately till 1795. Maclay seems to have anticipated men of science by many years in proposing to use the recession of the gorge as a scale for measuring geological time. Perhaps even more noteworthy is the unconcern with which he sets down his conclusion that the gorge was over 55,000 years old, at a time when geology was still confined by the narrow and seemingly impassable limits of traditional Biblical chronology. Maclay's informant, Ellicott, was a Government surveyor, and, according to the account of his life in Appleton's "Cyclopedia of American Biography," "he was selected by Washington in 1789 to survey the land lying between Pennsylvania and Lake Erie, and during that year he made the first accurate measurement of the Niagara river from lake to lake, with the height of the falls and the descent of the rapids." This early survey seems entirely unknown to geologists, who all refer to Hall's of 1842 as the first. Ellicott was a man of considerable attainments, a professor in West Point from 1808 to 1820, and the correspondent of European learned societies. He left some published writings, and works still in manuscript. Cannot the data of this survey of a century ago be found? If it proved a careful one, it would double the period of scientific observation of the recession of the gorge, and increase the certainty of any generalization. The map of Evershed's survey of 1883 was published in *Science*,

vol. v. p. 398. In *NATURE*, vol. xxxii. p. 244, Mr. Garbett quotes some interesting observations from the *Gentleman's Magazine*, January 1751, from Kalm's description of the Falls as seen shortly before. Mr. Garbett tries to identify an island that Kalm mentions as intersecting the Falls as Luna Islet. This would give a definite point for calculation. It seems to me that an attempt to test Mr. Garbett's conjecture by the Evershed map must show that it is untenable. If I have understood him, and correctly measured in accordance with his suggestions, the recession of the Canadian Falls will have been about half a mile in the 133 years, or about twenty feet a year. Notwithstanding Kalm's small dimensions, I think he meant Goat Island, for he says the island lies parallel with the river, and Luna Islet prolonged would lie almost at right angles to the river at its lower end.

EDWARD G. BOURNE.

Adelbert College, Cleveland, March 5.

Modern Views of Electricity.

I WILL reply to the last paragraph of Dr. Lodge's letter in *NATURE* of March 19 (p. 463) in his own language. I have no objection to contemplate a piece of isolated zinc surrounded by straining oxygen atoms, each negatively charged. And on the hypothesis that such atoms exist, I have no difficulty in realizing the depression of potential of the zinc, nor do I fail to appreciate the momentary transfer of electricity accompanying the sudden approach of the crowd of oxygen atoms when contact is made between the zinc and copper.

If we assume that atoms of oxygen have a negative charge, and if we further assume that, whether or not actual chemical combination takes place, they are attracted by zinc, and in a less degree by copper, then I admit that we have, on Dr. Lodge's principles, a consistent explanation of certain facts, viz. that, in an oxidizing medium at zero potential, isolated zinc has potential -1.8 volts, and isolated copper has potential -0.8 volt, and that, when metallic contact is made between them, both assume a common potential -1.3 volts. So far I understand, and with only slight modification can accept, the theory developed in "Modern Views."

But I cannot, on this hypothesis, account for the slope of potential assumed (p. 111) to explain the aluminium needle experiment. Suppose the zinc and its contiguous air film to be exactly inclosed by a surface S. There is then within S an excess of negative electricity. Dr. Lodge says it is wholly on the oxygen atoms within the film, and not on the zinc—which statement I accept. Then, at every point in external space we have lines of force converging towards S, as to any negatively charged body—that is, a slope of potential downwards towards S, and at all points not too distant this will be appreciable. When contact is made with the copper, the excess of negative electricity within S is diminished, and the slope of potential at any external point is diminished in the same proportion.

Another, and, I think, quite distinct view which Dr. Lodge seems disposed to entertain is, that the atoms (or molecules) of oxygen within the air film on the surface of the zinc, without having any actual charge, are polarized—that is, have one pole positively and the other negatively charged, and present the negative pole to the zinc. So that the zinc is inclosed in a shell exactly analogous, *mutatis mutandis*, to a closed magnetic shell with its negative face inwards. I admit that such an arrangement, could it exist, would possess the properties attributed by Dr. Lodge to the air film. It would have no influence at any external point, and would give a negative potential to the inclosed zinc. But a difficulty arises *in limine*. To polarize the atoms in this manner, or to arrange them in this manner if independently polarized, requires work to be done. And from what source is the energy derived, no actual combination taking place between the atoms in question and the zinc?

S. H. BURBURY.

Mud Glaciers of Cromer.

BETWEEN Cromer and Trimmingham, it is well known, the cliffs are largely made up of boulder clay and chalky loam, in the contorted drift. The lower part is, generally speaking, composed of more compact boulder clay, while the upper portion, sand and loam, is eroded backwards more rapidly by constant slipping; this slacks by the action of rain and springs, and becomes a feeding-ground for a number of mud-streams, which creep down the gulleys or fall as mud-avalanches over the