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THE OCEAN ECHO—HENRY VS. TYNDALL AGAIN.

Even his arduous labors in connection with the Centennial Exposition, added to his other pursuits, have not been sufficient to cause the venerable leader of American scientists to relax his researches into his favorite acoustical problems during the past year; and he recently came before the National Academy of Sciences with a new series of discoveries and theories, which he modestly announced as a "few additional facts" related to the results of his previous investigations.

It will be remembered that, at the 1875 fall meeting of the above named association, Professor Henry read a paper on a similar subject—in fact, his attention has been enlisted in the same direction for many years—in which he changed the scientific duel between Professors Tyndall and Osborne Reynolds into a triangular controversy. While he contented himself with disagreeing with Reynolds in many points, he hurled such a host of convincing experiments against Tyndall's theories of "acoustic transparency" that those structures, which Reynolds had already badly undermined, had little substantial support left them, even in the minds of those perplexed physicists who watched this war of the giants from afar, and who scarcely ventured opinions of their own in view of the disagreement of so learned a triumvirate of doctors.

Now Professor Henry returns to the fray, and again proceeds to discomfit the results of the "scientific use of the imagination" of Dr. Tyndall, not by propounding adverse theories, but by the inexorable logic of actual experiment. How he does it will appear in the following brief explanation of the new discoveries, which chiefly relate to the "ocean echo." Loud sounds, Professor Henry says, are wanting in analogy to light, so far as concerns obeying its rule that the angle of incidence is equaled by the angle of reflection. Instead of being reflected from a parabolic mirror in parallel rays, sounds diverge in all directions. A whistle being located in the focus of a parabolic reflector, 12 feet in diameter, gave a sound which, at a distance of 4 miles, had diverged so that it reached the whole horizon, and was heard with equal intensity to the rear and in front of the reflector. The cause of this divergence is explained in two ways: first, we may suppose the crest of a sound wave to be abruptly terminated at either extremity, when the tendency of the compressed air which constitutes the wave will be to expand itself in all directions—laterally from the ends of the wave as well as directly in front. Second, another cause may probably be found in the retardation of the two ends of the wave as it proceeds from the mouth of the trumpet. This would occasion a curling of the ends of the wave, as well as an elongation of them as they proceed from the swelling aperture. In the tendency of the sound to spread is to be found an explanation of the action of the trumpet, which gives the sound beam a greater condensation along its axis, and thus checks its spreading. Thus a speaking trumpet may act as efficiently if lined with felt as if lined with metal.

Although the tendency of sound is to diverge in all directions from an axis, yet there are cases where "sound shadows" are produced. Professor Henry mentioned a case where a fog whistle was placed near the water level of an island on which was a conical elevation. Vessels approaching from the other side of the hill heard the sound distinctly at a distance of three miles; but when the distance was reduced to a mile, the sonnd was lost and not recovered at any smaller distance. Here the termination of the shadow was at the one mile point, at which the diverging beams of sound, passing over the crest of the island, bent down and reached the surface of the water.

These conclusions are applied to the elucidation of the ocean echo, which is a reverberation coming from the horizon, near the surface of the ocean, and from around a point in the prolongation of the axis of the trumpet. It will be remembered that last year, in a lecture before the Royal Institution, Professor Tyndall adduced a number of brilliant experiments to show that echoes may be caused by reflection of sound from clouds of air of varying density. He showed, for example, that invisible warm air may act as an "acoustic cloud," and he pointed out that, "when such clouds are close to the source of sound, the echoes are immediate, and mix with the original sound; but if the acoustic clouds are further off, then there are prolonged echoes." He also showed the reflection of sound from gas flames. Professor Henry offers no objection to Dr. Tyndall's proof that a reflection of sound from a portion of air of different density is possible; but he says Tyndall's experimental conditions are exaggerated, and fail to represent any real atmospheric state. To test Tyndall's theory, he turned the mouth of a trumpet toward the zenith. The blast was intense, but no echo from the prolongation of the axis, that is, from the zenith, came back, although it was audible all around the horizon, half of which was on land and half on water. A rain cloud passed over the trumpet, and even a few drops fell: still no sound from the zenith. Compare this with Tyndall's experiment, in which he showed that, while two hundred layers of muslin did not cut off sound, a single layer, when wet, did, the latter presenting continuity of the air. Certainly it might be supposed that the rain cloud would act in a somewhat similar manner to the wet fabric. Professor Henry repeated his experiments several times, failing in each case to find any substantial basis for Dr. Tyndall's assumption. On the other hand, applying his own conclusions, he considers the echo to be due to reflection from the perfectly smooth surface of the ocean. On account of the divergence of sound, portions of waves in every direction must have descended to the horizon; and as some of these must have reached the plane of the ocean in a path curving inward toward the source of sound, they would, when they reached the ear of the observer in the vicinity of the source, seem as if coming from a point in the horizon, and hence would give rise to the phenomenon of ocean echo. Rays of sound at different distances from the ear would be reflected from the surface of the ocean, and thus occasion the prolonged echo: a blast of 5 seconds in one experiment on this point gave an echo lasting 20 seconds. "This," says Professor Henry, as a final shot at the "acoustic cloud" theory, "could only be produced by ordinary reflection from a series of surfaces placed at different distances, an arrangement of the material of the atmosphere which (on the doctrine of probabilities) would not be of frequent occurrence."

SLADE SUSTAINED.

Speaking of the exposure of the Slade trick, in London a few weeks ago, we expressed the belief that it would not lessen in the least the confidence of spiritualists in Dr. Slade or his practices. Even if strong enough to secure his conviction in the courts as a common swindler, the evidence of Dr. Lankester and others could not and would not shake their assurance of his personal honesty and the genuineness of his mediumship, for the simple reason that their confidence was the result of delusion, not a sane mental condition determined by or amenable to evidence.

Whether we were right or not as to the cause, we certainly were right as to the fact, for which we have the testimony of the president of the (British) National Association of Spiritualists. At a special meeting of the association, in London, October 4, that gentleman said he would willingly speak of Dr. Slade, in compliment to whom the gathering had been announced, but that could hardly be done without being drawn into a discussion of the case before the courts, and respect for the law made such a discussion unadvisable at that time. "It may be permitted me, however," he continued, with a sublimity of faith and felicity of diction marvelous to see, "it may be permitted me, however, to state a fact, which we cannot conceal if would, that our confidence in Dr. Slade as a genuine medium is in no way affected by the inferences drawn by two gentlemen who were quite inexperienced in the difficulties of the subject, and which inferences were founded on observations likely to be unconsciously vitiated by apparently slight but really important foregone conclusions"!

Surely our venerable poet must have been in a satirical mood when he penned the familiar lines:

"Truth crushed to earth shall rise again;
Th' eternal years of God are hers:
But error wounded writhes in pain,
And dies amid his worshippers."

Since the above was written Slade has been found guilty of trickery at his *séance* with Dr. Lankester, and sentenced to three months imprisonment with hard labor. From this decision, an appeal has been taken to a higher court, pending which he has been allowed to go out on bail. He was given the opportunity of performing his legerdemain in court, and of satisfying the judge of its spiritual character, but declined, not daring, apparently, to testify even in his own behalf.

THE LEVERRIER OF CHEMISTRY.

The correspondence between the hypothetical element eka-aluminum, imagined by the Russian chemist Mendeleef, and the real element gallium, recently discovered by M. Lecoq de Boisbaudran, is so remarkable that the attention of European scientists is now being closely devoted to its examination. In 1869, Mendeleef published a memoir, which attracted little notice at the time, but which announced as a law that "the properties of simple bodies, the constitution of their combinations, as well as the properties of the latter, are periodic functions of the atomic weights of the elements." Without entering into the details of the theories whence arose this conclusion, it will suffice to state that the author considers that this periodic law indicates the gaps which still exist in the system of known elements, and admits of predicting the properties of unknown elements, as well as those of their combinations. Thus, for example, there are two gaps in the groups D III and IV of the fifth series, which elements, yet to be discovered, M. Mendeleef some time ago named eka silicium (Es) and eka-aluminum (El). To show how this last mentioned hypothetical element is related to gallium, the characteristics of that metal must be reviewed.

At the present time, M. Lecoq de Boisbaudran has succeeded in preparing 7·5 grains. In a liquid state, gallium, the fusing point of which appears definitely to be 86·27° Fah., is of a fine silver whiteness; but on crystallizing, it takes a very marked bluish tint, and its brilliancy notably diminishes. By suitable cooling of the melted material, isolated crystals are obtained, in octahedral shape, and these M. de Boisbaudran is now measuring. As regards density, which is the important point to be noted, M. de Boisbaudran says: "In May, 1876, I attempted to measure the density of gallium by a specimen weighing 0·92 grain. I obtained 4·7 at 59° Fah. (and relatively to water at the same temperature). The mean of the densities of aluminum and of indium being 4·8 (to 5·1) the specific gravity provisorily found for gallium appeared to accord quite well with the theory placing that metal between indium and aluminum. The calculations established by M. Mendeleef, however, for a hypothetical body which appears to correspond with gallium, show the number 5·9. Gallium, crystallized under water, sometimes decrepitates on heating. Perhaps my first metal contained bubbles fill with air or water. To eliminate this possibility of error, I heated the metal highly and solidified it in a dry atmosphere. Then I obtained higher densities, varying from 5·5 to 6·2, the weight f the pieces tested being some tenths of a grain. Finally, I com bined six specimens, aggregating 8·7 grains." The mean of two different experiments gave (1st) 0 5935; (2d) 0·5956. "It is hardly necessary to insist," adds the author, "upon the extreme importance which attaches to the confirmation of the views of M. Mendeleef concerning the density of the new element."

This, however, is by no means all. Seven years ago, M. Mendeleef said, eka aluminum will have an oxide of the form El_2O_3." The oxide of gallium is Ga_2O_3. "It will be almost fixed, and will melt at a very low temperature This answers exactly to gallium, which melts at 86°. He said, further, that the future element, volatile and taking its place between indium and aluminium, would be discovered by spectral analysis, and so gallium was discovered.

We may agree with *La Nature*, whose editor, M. Tissandier, discussing this same subject, holds that Mendeleef's prediction abstracts nothing from De Boisbaudran's merit as the original discoverer. The French chemist attained his result in no fortuitous manner. He also foresaw the existence of gallium, and he isolated it only after ten years of persevering labor. He compared the spectra of different metals minutely, and thus was led to suspect the intermediate element between aluminum and indium.

The analogy between Mendeleef's discovery in chemistry and that of Leverrier in astronomy is most striking. Leverrier, from the perturbations of Uranus, deduced a hypothetical planet by purely theoretical considerations, treated it as if it were a real world, and then verified his calculations and theories by his magnificent discovery. Mendeleef likewise, by considerations as purely theoretical, conceived a hypothetical element. Had Adams, who discovered Uranus almost at the same time as Leverrier, worked from that astronomer's calculations, the analogy would be without a flaw, for he would then stand as De Boisbaudran now does toward Mendeleef. As it is, the discovery seems to open as wide an horizon in theoretical chemistry as did Leverrier's achievement in theoretical astronomy.

PANICS IN SCHOOL HOUSES.

It seems to us that remedial measures are needed to prevent the occurrence of the panics which, on the breaking out of a fire, real or imaginary, always occur in crowded schools, or at least to obviate the dangers incident to the headlong rush which takes place when the tumult overpowers the means of prevention. Several such scenes of confusion have lately been witnessed in this vicinity, and they are becoming sufficiently frequent to render parents unwilling to permit young and feeble children to attend the crowded public schools. A panic occurred the other day in a large school room, because a steampipe, leaking, discharged into the apartment a cloud of steam, which the children supposed was smoke from a fire. Another was just avoided through the scholars being at recess, when a genuine fire broke out near the recitation rooms, and on one of the stairways which formed a means of egress.

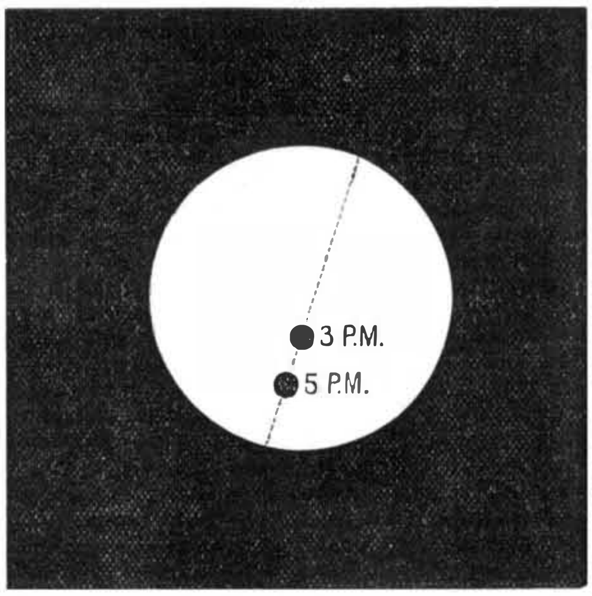
The prevention of disastrous confusion demands the greatest care, especially from those who construct school buildings and those who are responsible for their management. That such care is not exercised, we are persuaded from the frequency with which panics occur. Had the steam pipes been in proper order, or inflammable materials not existed in the school houses, neither of the above examples would have happened; and so, in every instance, some provoking cause can generally be found, which is attributable to a lack of proper vigilance or the absence of proper precautions. School houses should be fireproof and contain no material likely to feed flames. Even the probability of spontaneous combustion should be considered, and no dry or pulverulent material should be allowed to accumulate upon or around the steam heating apparatus. So carefully should risks be avoided that, while considering plans for new structures, or the introduction of new appliances into old buildings, the question whether there is anything in the schemes or projects proposed, which by any possibility might determine conditions sufficient to cause a panic, should be fully weighed. The case is one in which the ounce of prevention is worth a great many pounds of cure, although in the latter respect much can be done in providing ample modes of exit. If some rigid system of inspection of all school buildings, to be made by men thoroughly conversant with all the causes of schoolhouse panics—the principal of which, of course, is fire—were enforced, we probably should hear much less of children killed and injured through the efforts of a frightened crowd to escape.

VULCAN AGAIN.

Still another correspondent, as the subjoined letter shows, informs us that he saw Vulcan, or something on the sun which may answer to the description of that fugitive planet. This time the date is October 24, which is within Leverrier's first predicted period when observations should be made; and singularly enough the hour, 3 P. M., coincides with that of the reported observations of our other correspondents. The motion of the body, according to Mr. Wright, is different from that hitherto reported, as hitherto the spot has appeared moving upon the sun's disk, while now it is traveling off. The observation, in any event, is especially interesting, because it is the only one made since Leverrier's announcements in which anything resembling Vulcan has been seen; and moreover, it is also the only one of late date where the observer was not "taken by surprise," and where he adopted the best measures possible under the circumstances to verify the discovery. The following is the report:

To the Editor of the Scientific American:

I was reading last night in the SCIENTIFIC AMERICAN about the transit of Vulcan, and was so interested that I decided to make search myself during these clear, unclouded days. So this afternoon at 3 o'clock I got out my tube (4 inch lens); and at the very first focus the transit stood before me just as distinctly as it is printed in your paper (page 257). I was so astonished and delighted that I had to look and look again to satisfy my wondering eyes. Then I called my family, and they all saw it as plainly as I had done. Having satisfied myself by a half hour's observation that it had motion, I at once telegraphed to Professor Davidson (of the U.S. coast survey) at San Francisco: "Transit of Vulcan this afternoon; look for it." Then I went to the photograph rooms and used every effort to get a negative of it; but as we had no appliances, no facilities, and no knowledge how to take such a negative, it is not surprising that the one plate exposed is not of much value. The transit, however, was seen by four persons; others I did not summon, as I was more anxious to get a photograph. Had it been earlier in the day, I would have telegraphed to some eastern observers, but at the time I saw it first (3 P. M.) the sun was already set to eastern people; but I did the best I could by telegraphing to San Francisco, and then trying to get a photograph.



The apparent path of the planet, as near as my observations show, for the two hours' time before sunset, was as indicated on this diagram; and the time occupied in its transit I judge to be from about sunrise till 9 in the evening, about fifteen hours. In this diagram, I have drawn Vulcan's appearance too large: the relative size is perfectly shown in your diagram, page 257, this current month. W. G. WRIGHT.

San Bernardino, Cal., October 24, 1876.

If we might pin our faith to M. Leverrier's recent utterances, and assume, as we stated last week, that the supposed planet rotates about the sun once in 33 days, an even number of such periods from July 23, the date of other reported observations, would bring us to October 30, or within a week of Mr. Wright's observation. But M. Leverrier's views on the subject appear at present to be in a transition state, and our French mails each week bring us new statements from him, which of late have invariably failed to accord; in fact they often wholly differ from those enuciated seven days before. The reader will therefore understand that the data we now give, as well as those which we have presented, represent merely stages of progress in M Leverrier's investigation, through which we are endeavoring to follow him. The latest dictum of the eminent astronomer is more logical than some previous announcements, but at the same time seems to contradict flatly his previous results. In lieu of Vulcan swinging in a regular orbit in equal periods about the sun, we are now told that its orbit is highly eccentric, and that the planet behaves like Venus, making two transits within a few years, and then not repeating the passage for a century. This, of course, puts a stop to any such off-hand calculation of future transits as is above referred to.

M. Leverrier's reasoning whereby he reaches this conclusion is very interesting. He starts with the idea of finding a formula which will enable him to predict the Vulcanian transit, and to do this he makes use of Mercury, the theory of the motions of which planet, as is well known, is complete. Taking five good observations of Mercurial transits, dated 1789, 1802, 1832, and 1845, he determines this expression for the Mercurial orbit: $V = 56{\cdot}04^\circ + 4{\cdot}092307^\circ j - 7{\cdot}66^\circ$ sin. $v - 9{\cdot}18^\circ$ cos. v, in which j is counted from 1875. From this he calculated the next Mercurial transit, which he found would fall on November 9, 1848. Now this is exactly the date when a transit of Mercury did occur, and it was observed by Hind in London. In other words, had Mercury never before been seen, it would then have been discovered through the calculations.

M. Leverrier applies this method to Vulcan; and assuming the data of previous observers to be correct, he reaches the formula $V = 139{\cdot}94^\circ + 216{\cdot}18^\circ k + (10{\cdot}901252^\circ - 1{\cdot}972472^\circ k)j$, in which k is unknown, but the values of which are necessarily whole numbers. It is to be noted first that, if the solutions differ in the majority of points on the orbit, they coincide at the node, and this circumstance renders the problem much simpler. Besides, the variation of which k is capable is confined within very narrow limits.

With $k = 0$, the distance to the sun is 0·201, or one fifth of the earth's distance. The elongation is then 10°, that is to say, Vulcan is always so near to the photosphere that it is easy to understand why the planet is so rarely visible. With $k = 1$ almost the same results are obtained. The distance is not more than 0·181, and the rarity of observations is still better justified. But if $k = 2$, the rotation of the star must take place in 24 days, or in a less period than that in which the sun revolves on its axis; and consequently this solution is inadmissible, unless Laplace's cosmogonic hypothesis is rejected. Inversely, if $k = 1$ (or -1) in the above equation, the elongation becomes so great that for this reason the planet could not often be observed.

Now there has always been noted, in the transits of a single planet, periods of frequence and rarity. Venus, as we before stated, crosses twice in ten years, and then a century elapses before another transit occurs. The same is true of Mercury, and, M. Leverrier says, also of Vulcan. The period of the latter planet, he states, is 7¼ years, and there should be a transit on March 22, 1877, and not another until 1883. He advises that even the passage next year is not certain, the calculations showing that the trajectory of the planet will be sensibly tangent to the sun's edge; and besides, they do not determine its position with accuracy. But he counsels careful observations on the day noted.

Meanwhile there will be a chance for spectroscopists, as passages will occur frequently in the coronal region. These M. Janssen has already begun to search for.

IRREGULARITY OF THE EARTH'S MOTION.

Professor Simon Newcomb, of the Washington Observatory, is to be credited with a new astronomical discovery, which bids fair to be of some importance. He has found that our planet, instead of rotating regularly about the sun, is pursuing an apparently irregular motion, sometimes running ahead of, sometimes falling behind, the time based upon its own movement at any given period. The consequence is that the motion of the earth becomes no longer an absolutely exact standard for time measurement; and thus our reliance on our globe, already impaired since it has been demonstrated that there is no such thing as *terra firma*, and that its surface is constantly changing, is again weakened, and in a new direction. It is safe to believe that, now the discovery is in the hands of the astronomers, we may look for remarkable deductions

Professor Proctor, who has recently been discussing it, says that for about half a century there has been a doubt among astronomers as to the steadiness of the earth, and that Sir William Herschel suggested the possibility that, if a careful comparison were instituted between the turning motion of the earth and that of other planets, minute changes might be recognized. Accordingly he undertook the study of Mars, and measured the Martial day to a tenth of a second in a day; but this was of no use in testing the errors of our terrestrial time piece, where the same errors have to be measured by hundredths of a second in a year. Besides in Herschel's time the doubt on the earth's motion had been raised by Halley's recognition of the moon's apparent hastening; and this suggested little, because the lunar movements had never been closely analyzed, and the lunar hastening, as it was, indicated too small a change for Herschel to measure by his standards. Still this vague doubt was deemed of sufficient importance to cause Laplace to investigate it; and he showed that, among the various circumstances which affect the moon, there is one whose effect, at present and for many centuries to come, will hasten her motion. Then calculating the amount of such hastening, he concluded that it exactly corresponded with the hastening actually observed. "Perhaps there is not, in the whole history of Science," says Professor Proctor, "a more remarkable circumstance than this seemingly exact solution of a most difficult problem, where in reality the solution was incorrect." There was no forced agreement of figures; the work was placed in all its detail before the scientific world; mathematicians and astronomers recomputed it, and all agreed in its accuracy.

About a quarter of a century after Laplace's death, Adams (the co-discoverer, with Leverrier, of Neptune) re-examined the reasoning and found a flaw. Laplace judged a certain effect might be neglected. Adams thought not, and tested the matter; and then it appeared that it exercised so important an influence that, when due correction was made in Laplace's work, only one half the hastening was accounted for. Then arose a storm in the astronomical world. Leverrier, with all his acumen, failed at first to perceive the nature of the correction, and declared Adams to be mistaken. Pontécoulant sneered at it as "analytical legerdemain;" but the English mathematicians first accepted Adams' result, and then, after Delaunay had verified it, the continental astronomers followed. Delaunay not only admitted a retardation of the earth's motion, but pointed out where and how the same might be affected, namely, by the friction of the great tidal wave, which travels round in a direction opposed to the earth's rotation. This view has been generally accepted; and it can be shown that, if a clock could be made to go at a rate corresponding precisely to the earth's rotation, as indicated now, for 100 years, at the end of that time the earth would be found to have lost 22 seconds.

Now comes in Newcomb's discovery to show that the earth (judging from the moon's movements) undergoes irregular changes. It lost seven seconds between 1850 and 1862, and then, turning too fast between 1862 and 1874, gained eight seconds. Meanwhile smaller changes, some in one direction, others in the other, have taken place, generally lasting about four weeks at a time.

Two theories are suggested to account for these movements, either that the earth's motion is nominally irregular, or that some unseen body passes near enough to the moon to disturb her motion around the earth. Professor Newcomb adheres to the first hypothesis.

UP to the hour of going to press, the list of patents issued during the week ending October 17, and bearing that date, had not arrived from Washington.

SHELL lime, which contains considerable phosphorus, is superior to stone lime for agricultural purposes.