SCIENCE

NEW YORK, MARCH 6, 1891.

LIGHTNING-ROD PROTECTION.

What is the Problem?

IN seeking a means of protection from lightning-discharges, we have in view two objects,—the one the prevention of damage to buildings, and the other the prevention of injury to life. In order to destroy a building in whole or in part, it is necessary that work should be done; that is, as physicists express it, energy is required. Just before the lightning-discharge takes place, the energy capable of doing the damage which we seek to prevent exists in the column of air extending from the cloud to the earth in some form that makes it capable of appearing as what we call electricity. We will therefore call it electrical energy. What this electrical energy is, it is not necessary for us to consider in this place; but that it exists there can be no doubt, as it manifests itself in the destruction of buildings. The problem that we have to deal with, therefore, is the conversion of this energy into some other form, and the accomplishment of this in such a way as shall result in the least injury to property and life.

Why have the Old Rods Failed?

When lightning-rods were first produced, the science of energetics was entirely undeveloped; that is to say, in the middle of the last century scientific men had not come to recognize the fact that the different forms of energy — heat, electricity, mechanical power, etc.— were convertible one into the other, and that each could produce just so much of each of the other forms, and no more. The doctrine of the conservation and correlation of energy was first clearly worked out in the early part of this century. There were, however, some facts known in regard to electricity a hundred and forty years ago; and among these were the attracting power of points for an electric spark, and the conducting power of metals. Lightning-rods were therefore introduced with the idea that the electricity existing in the lightning-discharge could be conveyed around the building which it was proposed to protect, and that the building would thus be saved.

The question as to dissipation of the energy involved was entirely ignored, naturally; and from that time to this, in spite of the best endeavors of those interested, lightning-rods constructed in accordance with Franklin's principle have not furnished satisfactory protection. The reason for this is apparent when it is considered that this electrical energy existing in the atmosphere before the discharge, or, more exactly, in the column of dielectric from the cloud to the earth, above referred to, reaches its maximum value on the surface of the conductors that chance to be within the column of dielectric; so that the greatest display of energy will be on the surface of the very lightning-rods that were meant to protect, and damage results, as so often proves to be the case.

It will be understood, of course, that this display of energy on the surface of the old lightning-rods is aided by their being more or less insulated from the earth, but in any event the very existence of such a mass of metal as an old lightning-rod can only tend to produce a disastrous dissipation of electrical energy upon its surface,— "to draw the lightning," as it is so commonly put.

Is there a Better Means of Protection?

Having cleared our minds, therefore, of any idea of conducting electricity, and keeping clearly in view the fact that in providing protection against lightning we must furnish some means by which the electrical energy may be harmlessly dissipated, the question arises, "Can an improved form be given to the rod, so that it shall aid in this dissipation?"

As the electrical energy involved manifests itself on the surface of conductors, the improved rod should be metallic; but, instead of making a large rod, suppose that we make it comparatively small in size, so that the total amount of metal running from the top of the house to some point a little below the foundations shall not exceed one pound. Suppose, again, that we introduce numerous insulating joints in this rod. We shall then have a rod that experience shows will be readily destroyed — will be readily dissipated — when a discharge takes place; and it will be evident, that, so far as the electrical energy is consumed in doing this, there will be the less to do other damage.

The only point that remains to be proved as to the utility of such a rod is to show that the dissipation of such a conductor does not tend to injure other bodies in its immediate vicinity. On this point I can only say that I have found no case where such a conductor (for instance, a small wire or gilding) has been dissipated, even if resting against a plastered wall, where there has been any material damage done to surrounding objects.

Of course, it is readily understood that such an explosion cannot take place in a confined space without the rupture of the walls (the wire cannot be boarded over); but in every case that I have found recorded this dissipation takes place just as gunpowder burns when spread out on a board. The objects against which the conductor rests may be stained, but they are not shattered.

I would therefore make clear this distinction between the action of electrical energy when dissipated on the surface of a large conductor and when dissipated on the surface of a comparatively small or easily dissipated conductor. When dissipated on the surface of a large conductor, — a conductor so strong as to resist the explosive effect, — damage results to objects around. When dissipated on the surface of a small conductor, the conductor goes, but the other objects around are saved.

A Typical Case of the Action of a Small Conductor.

Franklin, in a letter to Collinson read before the Royal Society, Dec. 18, 1755, describing the partial destruction by lightning of a church-tower at Newbury, Mass., wrote, "Near the bell was fixed an iron hammer to strike the hours; and from the tail of the hammer a wire went down through a small gimlet-hole in the floor that the bell stood upon, and through a second floor in like manner; then hori-

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LETTERS TO THE EDITOR.

**** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

The editor will be glad to publish any queries consonant with the character of the journal.

On request, twenty copies of the number containing his communication will be furnished free to any correspondent.

The Threatened Abandonment of the National Zoölogical Gardens.

A LITTLE over a year ago it was the source of the very greatest gratification to American science that the bill before Congress had passed, and a liberal initial appropriation had been made to establish a national zoölogical garden at the seat of the general government at Washington. Outside of strictly scientific circles, thousands upon thousands of earnest sympathizers all over the country likewise rejoiced in the success of the movement. The great mass of intelligent and cultured people of this nation felt a secret satisfaction when the broad project took on shape and became a living fact. Thoughtful men, wise and far-reaching minds, felt it to be one of the best indices of our national growth, culture, and civilization; for we well know that the nations of the world most distinguished for such characters invariably support such institutions, as they do, indeed, great libraries, galleries of art, and the museums.

To-day it is with deep concern that the intelligent well-wishers — and their name is legion in America — regard the miserable wrangle that is now being enacted in Congress over this entire matter, — an ill-directed debate, that, as it proceeds, daily enhances the danger of defeating the entire measure, undoing all the good that has been done. Nor is this feeling of concern confined to this country; for science the world over deplores the present state of affairs just as much as we do, for there is a broad freemasonry among those who have at heart the progress of learning, the aims of general education, and the advancement of any step that promotes a truer civilization.

But, upon my word, I am almost constrained to believe sometimes that the *personnel* of this government of ours really believes that we have arrived at such a high pitch of civilization in the United States that we are above all such matters: in fact, we are living in an atmosphere far above such questions as the maintenance of public libraries, zoölogical gardens, national universities, or museums.

Viewed from this point, it is a delightful thing to contemplate the marvellous rapidity with which our present-day civilization is advancing. To touch upon a few practical points in the question now under consideration, the writer is moved to say, and I believe I voice the opinions of many other scientists beside myself, that the greatest praise was due to Mr. W. H. Hornaday and Senator Beck for their unflagging energy in carrying through Congress the bill to establish our National Zoölogical Gardens; that the people of the District of Columbia, and of Washington in particular, lent their most hearty aid in the premises, as would any other honest and patriotic American city in the same place, and now it is an outrage to expect her to support any part of what purports to be a purely national enterprise; that the Rock Creek Park is one thing, and the National Zoölogical Garden is another; that, as highly important as an astro-physical laboratory is, and notwithstanding the evident demand for such an institution, it surely has nothing to do with a zoölogical garden, any more than the moon has to do with the beard on the chin of a buffalo; that the time has most assuredly arrived for this country to establish, support, and maintain a complete, extensive, and properly conducted national zoölogical gardens at the seat of her general government, — gardens that can at least rival those of Regent's Park in London, or the superb ones maintained at Amsterdam; and assuredly nothing less, or none at all. My views upon the conduction of such establishments, together with their aims and uses, have already been published in *The Popular Science Monthly* of New York (April, 1889), and those views were very fully republished in *The Evening Star* of Washington, D.C.: so it is quite unnecessary to touch upon that part of the subject again in the present connection. R. W. SHUFELDT.

Takoma, D.C., Feb. 26.

A Water-Beetle.

LATELY I kept for a few days for inspection that very beautiful insect a water-beetle. The specimen was large and splendidly colored, gold-banded, and displaying brilliant iris hues on its legs. I placed it in a glass jar of water. On the surface of the water some leaves were laid. On one side of the jar, at the bottom, was pasted a square of paper, and to the shelter of this the beetle often retired. It seemed to take the greatest delight in darting, swimming, and diving, rising from the bottom of the jar to the top of the water by long, vigorous strokes of its hind-legs. Then joining its second pair of legs before it, like a swimmer's hands, and stretching the hind pair out nearly together, it would dive to the bottom. It slept hanging head downward under the leaves, with the tip of the body above the water to secure air.

It showed the pleasure of a child in "blowing bubbles." Rising to the surface, it would put the tip of its body above the water, part the elytra, and take in air; then, closing its case, it would dive to the bottom, stand on its head, emit the air-bubble by bubble until it was exhausted, and come up for a new supply. It seemed to need the daily renewal of the water in the jar. When it was hungry, or the water was not fresh enough, it became dull and sulky, and hid behind the paper. After the beetle had fasted twenty-four hours, I laid on the top of the water a wasp, a mosquito, a blue-bottle fly, and a common fly, all dead. The beetle, being at the bottom of the jar, did not seem to see or smell these insects. Rising presently, he came up against the mosquito, seized the body in his jaws, and sucked it dry with one pull. He then found the blue-bottle, carried it down to the shelter of the paper, trussed it neatly, cutting off the wings, legs, and head, and letting them float to the surface. He then held the body in his hands, or short front-feet, pressed to his jaws, and sucked it dry. After this he rose to the surface, found the other fly, and served it in the same fashion. Next he found the wasp, a large one. Carrying this below, as he had the flies, he clipped off the wings and legs, but took the precaution to suck the head and thorax before turning them adrift. He also grasped the body in his hands, pressed the part that had been cut from the thorax to his mouth, and, holding it exactly as if drinking out of a bottle, he drained it dry.

I found that he could eat all the time, except when he was asleep or playing, and his activity was in proportion to the quantity of his food. Cooked meat he would none of. Raw beef he did not greatly like, but raw veal he prized even above wasps and blue-bottles. I cut an ounce of raw veal into dice, and dropped it in the bottom of the jar in a heap. He did not seem to see or smell it, but after a while happened to dive into it. He appeared to be full of joy at the discovery. One fragment after another he took in his hands, held it closely to his jaws, and sucked it dry by strong pulls. At each pull I could mark the receding red juice of the meat. When the veal was reduced to a pale fibre, he let it go and took a fresh bit. He always retired to the shelter of the paper to eat, with the sole exception of the mouthful he made of the mosquito. Like the King of Dahomey, he would not eat in public. JULIA MCNAIR WRIGHT.

Fulton, Mo., Feb. 26.

Cold and Warm Waves

THE observations taken at the meteorological establishment on the Eiffel Tower in Paris have led to several most interesting results; and among other things it has recently been found that the velocity of the air during an ordinary strong wind is about twice as high at the top of this tower as it is at its base. Such being the case, we should expect to find advancing cold or warm waves far ahead in upper regions of what they are closer to the earth's surface: and so they actually are found to be, as mentioned by Professer Hazen in your last issue, when he says that the temperature change at isolated mountain-peaks, as Mount Washington or Pike' Peak, occurs several hours earlier at their tops than at their bases, or when he says that high areas, etc., advance with a velocity double that of the surface air. These phenomena give us, therefore, a very instructive illustration or proof of the effect of the friction between the earth's surface and the air moving over it; and they confirm the old popular belief that weather-changes are brought about by the wind, or, what amounts to the same thing, that the advance of cold and warm waves is entirely due to mechanical action, or displacement of the surface-air as a body, in conformity to such rules as I have set forth in my paper, "On the Cause of Trade Winds" (*Transactions of the American Society of Civil Engineers*, vol. xxiii.), which paper also gives a very simple clew to the increased cold or heat in the border current of cold and warm waves.

Professor Hazen, however, does not appear to be acquainted with the important results of these observations at Paris, when he concludes that the changes in temperature and humidity of the air accompanying the advance of these waves cannot be due to the wind, or are entirely independent of the motion of a mass of air, although he curiously enough states at the same time that a rapid motion of an advancing wave has a tendency to increase the wind, which seems contradictory.

Starting from these false premises, no wonder our meteorologist arrives at some most startling results. He finds that the moisture of the air is "removed," "eliminated," or, as he says elsewhere, "sucked out" of the air in less than no time by some mysterious agency or another which cannot as yet be accounted for. Storms are transported or transferred through the air without the air-particles being moved at all. Indeed, when it is considered that the literal meaning of the word "storm" is "violent agitation or commotion," or, in other words, "wind," he wants to tell us that when a wind blows, the air-particles don't move at all: it is all deception, and the storm is due to electric energy or something else. The professor's mistaken notion here is, however, precisely similar to the one I pointed out in my last letter, when I tried to explain the fallacy of the result he arrived at,— that condensation did not always take place when saturated air "got chilled." His ideas of the principles of motion seem to differ remarkably from those engineers are accustomed to go by.

Finally, an entirely different subject is brought up by him, and treated in the same mysterious manner: "A portion of the heat in our storms is due to a peculiar condition of the atmosphere which intercepts the heat of the sun, and this heat gradually works down from the upper atmosphere to the earth." Mightn't it be simpler to say that when the sun is prevented from warming the earth's surface, its heat is taken up by the clouds, and consequently, when the cloud-carrying layers are brought near the earth's surface, as we know they are towards rain, this heat is felt by us?

Professor Hazen is a meteorologist without a theory; and, although it may be much easier to run down than to build up, no doubt he has done excellent service by constantly finding fault with others in just conformity to this negative standpoint; but, as the professor always seems so very anxious "to strike at the very heart of present theories of storm-generation," and this evidently in his strong point, I may recommend him to strike at the heart of a rain theory I some time ago had the honor of presenting to the American Society of Civil Engineers, and he may thereby possibly be able to prove that his notions of the principles of motion, etc., are more correct than those held and practised by the members of that distinguished body.

FRANZ A. VELSCHOW, C.E.

Brooklyn, N.Y., March 2.

The Piney Branch Indian Workshop.

THE "Annual Report of the Curator of the Museum of Archæology, Philadelphia" (Vol. i. No. 1) contains a criticism of recent work done, and conclusions drawn, by Mr. W. H. Holmes of the Bureau of Ethnology at the Piney Branch Workshop, near Washington, D.C., and of Mr. Holmes's papers thereon (*American Anthropologist* of January and July, 1890), that to the writer appears to do great injustice to Mr. Holmes.

In his report, Dr. Abbott, who has visited the site and obtained specimens therefrom through Mr. Holmes, says, "The enormous number of 'blocked out' implements have recently been held as conclusive evidence that such objects are to be considered as 'failures,' and, this being so, that similar objects found under any circumstances in this country are of like signification." To such conclusion the doctor dissents (p. 8).

Again he says, "While the position taken by Mr. Holmes and others as to the archæological significance of the Piney Branch deposits may be wholly correct, and stand the test of every objection, the inferences drawn are too sweeping, and have not necessarily the bearing upon the question of man's antiquity in America which he practically claims. The conditions under which rude paleolithic implements occur in the valley of the Delaware are wholly different. Here they are characteristic of a horizon; are so associated with a well-marked deposit, that by no verbal jugglery can they be relegated to 'incongrous association,' and so are adventitious" (p. 9).

And concluding, the doctor says, "On the other hand, to accept Mr. Holmes's conclusion, that all rude implements, howsoever and wheresoever found, are Indian 'failures,' is not merely to remove from the class of implements the so-called 'turtle-backs' of the Delaware valley, but to remove the paleolithic implements of Europe, Asia, and Africa from the prehistoric archæology of those continents."

Mr. Holmes is an officer of the Bureau of Ethnology, whose works on pottery, on the antiquities of the South-West, and on the Chiriquian objects, have familiarized his name to all students of American archæology as a most painstaking and careful investigator; and, had he taken the ground asserted, he would have laid himself open to the charge of want of due care in conducting a scientific work.

Thus it will be observed that Dr. Abbott first says the Piney Branch objects "have recently been held as conclusive evidence that such objects are to be considered as failures," and dissents from such conclusion. Again he says, "Whilst the position taken by Mr. Holmes and others" may be correct as to Piney Branch, the conclusions are too sweeping, and have not the bearing which he (Mr. Holmes) practically claims. And in conclusion, Dr. Abbott, while claiming that the discovery of paleolithic implements of the Delaware valley occurred under different conditions from those under which the implements at Piney Branch were found, says the Delaware valley implements "by no verbal jugglery can be relegated to 'incongrous associations.'" The report starts by saying that the Piney Branch objects "have been held," and, later on, by "Mr. Holmes and others." In the last part of the latter sentence in which "Mr. Holmes and others" occurs, the doctor, in specifying Mr. Holmes individually, saddles the latter with conclusions which began with "have been held," and then defends the paleoliths of the Delaware from being by "verbal