man (president, we might say) of this forest republic, which comprised the thirty confederated tribes of Powhatan." These questions need not be discussed now, but such statements ought not to be made history.

My attention was directed to the article by a friend, who was surprised by a mention of the snow-snake among these primitive Virginians. The children indoors were playing at *gus-ha-eh* (or 'peach-pits'), it is said; but where the peaches came from at that early day is not explained. The "boys were tumbling about in the light snow, at their favorite game of *ga-wa-sa* (or the 'snow-snake')." The use of these Seneca words sufficiently shows the source of information, but it is not wise to place a Northern game so far South. Something more than a light snow is required for this sport; and boys do not tumble about in it, but stand up to their work. Neither would any one risk breaking the graceful shaft between the legs of one running at full speed.

I was recently surprised at not finding the snow-snake in the collection of Iroquois implements at the Museum of Natural History in New York, and still more to learn how few students of Indian life know any thing of it. A game something like it is found among some Western Indians, but the implement used is very different. Nor do I now recall any mention of it among early Indian games. That it would not be of general use is in the nature of things. Only in those colder regions where an icy crust often forms would it naturally occur. Even there it may be recent, as the head is always weighted with metal, melted into grooves, and nicely smoothed. Of course, this might have been different if its use was ancient. Morgan describes the Seneca snow-snake as being from five to seven feet long, and he gives a good figure and description. Those of the Onondagas are often longer. Mine is of their medium

size, and is upwards of seven feet long, while I have seen many not less than nine feet in length. They are very neatly made, for any irregularity would interfere with successful use. The smaller notched end of mine is but a trifle over half an inch wide by about a quarter deep. This increases to seven-eighths of an inch wide by one-half deep just back of the raised head at the other end. The head begins to rise about six inches from the extreme point with a gradual curve. Lead is inlaid in this, often in a pretty pattern; and I have thought it barely possible, granting its antiquity, that stone bird amulets may once have formed the heads. These seem to belong to woodland regions, where the winters are long, but such a use is hardly probable.

The game is simply one of dexterity and strength. The forefinger is placed in the basal notch, the thumb and remaining fingers reaching along the shaft, and the snow-snake is thrown forward on the ice or hard snow. It might go a little way through light snow, but this is not favorable to its use. An icy crust or the track of a sleigh, the travelled road, or even ice, are favorite resorts. A much worn road would injure the fine polish of the implement, and a level surface, with a good crust, is commonly chosen. On a fine winter's day men and boys are often seen in such places, pursuing this sport. They play quietly, for the Onondagas are a very quiet people, and one out of sight might know nothing of the most exciting game. When the slender shaft is thrown, it glides rapidly over the surface, with upraised head and a quivering motion, that gives it a strange resemblance to a living creature. The Christian Onondagas have abandoned its use, perhaps because betting is a feature of the game, or it may be they dislike its symbolism; but I think the former the true reason. The Senecas call it *ga-wa-sa;* the Onondagas, *ka-wher-tah;* neither of these words referring to its snaky appearance. I am unable to learn of any idea attached to the name, and this favors an early use. The game is to see which person or side can throw it farthest, and sometimes the distance of a quarter of a mile is reached under favorable circumstances, but I think this rare. W. M. BEAUCHAMP.

Baldwinsville, N.Y., Dec. 30.

The Conspiracy of Silence.

THE Duke of Argyll's charge of a conspiracy of silence among scientific men, by means of which new truths are to be ignored, has been perhaps sufficiently answered. In fact, according to the duke's own statement, the theories of Messrs. Murray and Guppy are already printed, and are before the public for judgment. This discussion has been interesting, but, so far as I have seen, two points worthy of attention have not been brought out.

1. There is in all branches of learning a just and good conservatism. We cannot afford to give up scientific truths that have been acquired with much labor and difficulty. Hence, when theories are proposed that conflict with established principles, they are to be received with caution. No one can believe in perpetual motion until our theory of dynamics is overthrown. A mathematician who claims to solve the equation of the fifth degree will have a hard time in finding believers. If a writer on theoretical astronomy violates the rules of the calculus, he has no right to ask the respect of astronomers. He deserves to be ignored. Astronomers should not spend their time in demolishing absurd theories that may be proposed to them. The age of Don Quixote is past.

2. But in the discussion which has taken place the assumption seems to be made that scientific men are better, or ought to be better, than other people. Although this might be taken as a tacit compliment, I think it is a mistake. The truth is, that scientific men are very much like other people. They have the same desires, the same passions; and they will have the same greed for money and fame that other people have. If they place themselves on a footing devoid of morals, they will develop as mean men as the world has ever seen. But it is not simply from the character of the men who do scientific work that we are to look for good results. These come rather from the scientific method, which, in its final judgment, pays no regard to the condition of the worker. The question is only if his result is right. The dissipated young Frenchman, Galois, was killed in a duel at the age of twenty-one, but his genius was so powerful that he left an indelible mark on the old science of mathematics. His work remains, and in using it we do not consider Galois and his extreme republican principles. ASAPH HALL.

Washington, D.C., Jan. 10.

One of the Causes of the Inefficiency of the Reis Telephone.

SOME who have experimented with the Reis telephone declare that they have never been able to hear a transmitted word. Others have heard some words and sentences; but these have always been weak and irregular, so as generally to discourage one in a short time, especially now, when through the improvements in telephones it is possible to reproduce words both loudly and regularly. Experimenters therefore have been impatient with Reis's apparatus, and seldom have done any thing with it, except make some hasty tests for some phase of the great telephone controversy.

The inefficiency of the Reis telephone has, by a kind of common consent, been admitted to be altogether due to the imperfect mechanical operation of the transmitter, by which the making and breaking of the current when it is in operation is such as not to properly follow the actual vibratory movements of the diaphragm when the latter is moved by speech-vibrations; that at best it can deliver to the line only the fundamental rate of the vibration, leaving out the characteristic over-tones which are supposed to be necessary to the successful transmission of speech. This judgment as to the mode of operation of the transmitter has been derived wholly from what has been heard by one listening at the receiver; for there is to-day no known method by which it may be determined whether or not a transmitter has the proper motions, except by listening at the receiver. That is the test. Hence it has been concluded, that, if speech was not properly delivered in a receiver, the trouble must be with the lack of proper movements of the transmitter. Yet it is mechanically possible for the transmitter to move properly, and the receiver to be so much overloaded, so to speak, that the latter fails to be heard on account of the extra disturbance.

The Page effect — the magnetic click — may be so strong in a Reis receiver, with a proper current, as to be heard a good many feet distant from it. When the receiver is held against the ear, the sound may be very loud; so much so as to quite drown weaker

sounds, if they happen to be present. Especially when these loud sounds occur fifty or one hundred or more times per second, the effect is that of a continuous sound; and as the persistence of hearing is something like the tenth of a second, it follows, *a priori*, that such rates of vibration as from two hundred to a thousand per second might be present, yet too weak to be heard in the presence of such overpowering sounds that have an appreciable persistent effect. These loud magnetic clicks are heard only when there is a sudden break in the current in the receiver. If, then, some way can be devised for preventing these extraneous sounds in the receiver without interfering at all with the transmitter or its 'mode of operation,' one may experimentally determine whether the Reis transmitter does or does not act mechanically so as to vary the current in correspondence with speech or other sound-vibrations. I therefore conceived, that, if there was a short-shunt circuit between the terminals of the transmitter, some of the current would traverse the coil of the receiver the whole time, no matter whether the circuit through the transmitter was open or closed. The loud clicks would be suppressd without interfering in any way with the 'mode of operation' of the transmitter; and, if the latter really did follow the motions of the diaphragm, the variations in the current strength would correspond, and the speech would be heard. This I found to be truly the case: for with a transmitter thus provided with a shunt circuit of about two ohms, which could be switched in or out with a key, it was at once possible to hear a large part of what was spoken when the shunt was in; when it was out of circuit, the sounds were generally inarticulate.

This experiment is an *experimentum crucis*, and proves that the inefficiency of the Reis telephone is much more due to the extraneous sounds in the receiver than to the lack of appropriate motions of the platinum terminals of the transmitter. It proves that the transmitter does and must always have worked in the proper mechanical way, and that the current theory of its mode of operation is not correct. It proves, too, that when carbon is substituted for the platinum terminals, there is an improvement in efficiency, but not in its mode of operation.

A. E. DOLBEAR.

College Hill, Mass., Jan. 14.

Queries.

23. DROPS OF WATER. — Will some reader of *Science* explain the floating of drops of water upon the general surface? It is a very common phenomenon, not to be confused with the formation of bubbles, though often produced by the same sort of agitation; for example, the breaking of a wave. In still water they may be produced by an oblique blow with an oar or with the hand, but will disappear as soon as their original momentum has been lost. Recently on Lake Pontchartrain, with a brisk wind which kept them in motion, I observed some which exceeded an eighth of an inch in diameter, and lasted more than two minutes. The depression, like that of a floating needle, which surrounded each one, was also plainly visible. Their behavior was in striking contrast with that of the bubbles with which they were mingled, the drops moving much more rapidly with the wind, and also rolling under the influence of gravity towards the trough of each successive wave.

E. J. POND.

New Orleans, Jan. 9.

Answers.

21. GLOBULAR LIGHTNING. — The note on globular lightning in the issue of Dec. 30 recalls to mind a phenomenon of the kind I witnessed some years ago. While walking upon the Worcester and Norwich Railroad track about a mile south of Worcester Junction, I suddenly saw a ball of fire, or what looked like it, about the size of a large marble, running along on top of one of the rails just ahead of me. It was going at so slow a rate that I could have overtaken it in a few seconds, and my first impulse was to do so; but the sober second-thought warned me against making the attempt. I, however, watched it move until it came to the end of a rail separated from the adjacent one by something like half an inch, when it stopped, and in a second or two vanished, when there was a clap of thunder in a cloud overhead which I had not before noticed as being a thunder-cloud. The brightness of this small ball was not excessive, nothing to be compared with an electric arc. It was more like that of a red-hot bullet. It did not scintillate or make any noise, that I noticed. Now, while this was an accompaniment of a thunder-cloud, as are such manifestations generally, I think there is some reason for not calling the phenomenon itself an electrical one in the same sense as lightning is electrical. If electricity can gather itself up into a spherical form as if it was subject to some sort of cohesion, and if it can roll along on top of a good conductor instead of traversing the body of the conductor subject to Ohm's law, then there are some exceptions to this latter law. Other observers have seen still larger balls roll slowly upon the ground, or move with great deliberation in the air, apparently without exhibiting the property of attraction or repulsion. Some years ago an acquaintance in southern New Hampshire told me that such a fire-ball came down to the ground near his house, and rolled slowly about near where a hog was. The hog walked up to it as if to root it along, and touched it with its snout, when it exploded with a great noise, killing the hog instantly, blowing it to pieces. In this case, and in other similar ones, it appears that the luminosity is not caused by high temperature. Babinet reports a case that he investigated, where a globe of fire came into the room of a tailor who was eating his dinner. It was about the size of a child's head, and moved about upon the floor, approaching his legs as a kitten might have done; but he prudently drew his feet away, and watched it. It appeared bright, but the tailor said he felt no sensation of warmth. After remaining several seconds upon the floor, it rose vertically five or six feet in the air, and then moved towards a pipe-hole in the chimney, which was covered with paper, which it tore off, and went up the chimney. Near the top it exploded, and did considerable damage to the chimney and the roof of the house. Such a performance is entirely unlike electrical phenomena. It exhibits none of the characteristics of electricity, either in form, in motion, in heat, in attraction, and why should it be called an electrical phenomenon? It is true, in most of the cases reported the disruption of the globe resulted in electrical phenomena, sudden and destructive; but so would a charged secondary cell, that might have relatively a very large amount of potential electricity in it: that is, the stored chemism may be transformed into electricity at a very rapid rate, but we do not now consider that electricity is stored in the cell, because we can get a large amount of electrical energy out of it. The charged secondary cell is the result of electrical work; but, so long as the energy is stored in such a way as to manifest none of the properties of electricity, it is improper to speak of it as other than chemism. In like manner, if energy be stored in such a globular form as is called 'globular lightning,' which does not exhibit any of the properties of electrical energy, it seems to me that we are not justified in calling it an electrical phenomenon. We have in physics already too many *ex post facto* terms, such as 'heat' rays, 'light' rays, and 'actinic' rays; and it is a pity to call this 'globular lightning' if the only appropriate part of the name is 'globular.' If we don't know what specific form the energy exists in, we know that it is globular energy; and 'globular vim' would be better than 'globular lightning.'

A. E. DOLBEAR.

College Hill, Mass., Jan. 12.

22. WASP-STINGS. — In answer to Mr. Ames's query in *Science* for Jan. 13, I will say that I have picked up hundreds of lively wasps, holding my breath at the moment when the wasp was grasped, and have never been stung under such circumstances. I have frequently been stung by wasps when I have disturbed them unawares, which shows that there is nothing in my make-up which would prevent wasps from stinging me if they had the power and were so disposed. I have captured a dozen wasps, one after the other, until I had a handful, which I have held as long as I chose, without receiving a single sting. I cannot say that I would have been stung had I not held my breath at the moment of contact with the wasps, but can only testify that I was not stung in a single instance when I did hold my breath. As to the explanation of the phenomenon, I have none to give. I have tried the experiment on hornets, honey-bees, and bumblebees, and a single trial with each was sufficient to prove that the plan did not work with either of these species.

FAYETTE SAFFORD.

Willimantic, Conn., Jan. 16.