without any condensation, and the other phenomenon requiring a greater density in matter than exists in free space, may, perhaps, receive other explanations that do less violence to our ideas. Ether, in which the complex molecules of matter are entangled, certainly might act as if it were more dense without really being so.

What the experiment of Michelson and Morley seems to show is that the ether is swept along by the water, but lags behind. The question of density appears to me still to be an open one. Maxwell's experiment with a prism which was, as was then supposed, moving through ether at a speed of 18.6 miles per second, seems to have a very different relation to Fresnel's theory if the ether at the earth's surface is moving with it.

It does not seem hopeless to repeat the experiment of Michelson and Morley on a railway coach, with water or carbon bisulphide at rest in the tube, if the road-bed and the car selected are of the best construction, and the apparatus is elastically supported.

It would be necessary, probably, to rigidly connect the observer's seat and the water tube, and to support them, with the observer, by helical steel springs surrounded by rubber tubes filled with glycerine to dampen the vibrations.

A speed of forty miles per hour will more than compensate for the suppression of one water column, which will be replaced by air. This is precisely the form of experiment upon which Eisenlohr's analysis is based. In this form the conditions of the experiment are capable of great variation. The car becomes really the moving body, and the transparent region within through which the light passes, may be shielded by any kind of opaque matter. Whatever the results may be, they can hardly fail to add greatly to our knowledge of the effect of moving masses upon the luminiferous ether.

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.*

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

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

Jugglery.

IN *Science* for Aug. 14 there was an inquiry, quoted from *Illustrated News of the World*, as to the source of a certain statement regarding the apparently marvellous feats of Indian jugglers. In this statement it had been suggested that the spectators had been hypnotized by the performer, and hence imagined they saw something which the "snap-shot" of a kodak proved did not exist at all. I remember reading this very circumstantial account in an evening paper, and cut it out to send to India. After some search I have found the original reference. The story, as a quotation from the Chicago *Tribune*, was published in the *Evening Star* of Washington, D.C., on Aug. 30, 1890. Its author, Frederick S. Ellmore, purported to be a graduate of Yale College, and to have travelled extensively in India with an artist friend, a Mr. Lessing. It has since transpired that no person by this name is a graduate of Yale.

To my mind the story shows a good many signs of being on the Mulhatton style, and could easily have been written by some one who had never been in India. It is very plain that no juggler could by any possibility hypnotize a mixed audience all the time changing. Those who have seen the original growth of the mango-tree under the manipulations of the performer, who was stark naked except for a *lungooti* (breech cloth), will be inclined to smile at Hermann's explanation given in *Science*. My father has spent twenty years in India, and has seen this performance repeatedly. He has noted one singular coincidence, in that the tree is never made to grow except in the season when the leaves and fruit of the mango-tree are in proper order for the exhibition.

H. A. HAZEN.

Washington, D.C., Aug. 18.

The Rain-Makers.

EVERY reader of *Science* has seen the recent telegram from Midland, Texas, Aug. 11, "Preliminary explosions made yesterday; raining to-day." It may be well, with the apparent brilliant success of this remarkable undertaking before us, to examine this question at length.

Ever since the time of Plutarch the idea has been prevalent that great battles are invariably followed by rain. In the earliest times, before the introduction of gunpowder, it was thought that exhalations from the dead bodies might assist in precipitating the moisture, but in more recent times there has been a well-nigh universal belief among soldiers that heavy cannonading or firing will produce rainfall. Whence comes this common thought were there not a fact to originate and back it up? We may as well ask, whence comes the well-nigh universal belief that the moon has a marked influence upon the weather? Now it is well known that in the latter case, most careful researches extending over a century have shown either no effect at all, or one that was either contradictory in different periods, or almost inappreciable.

Now since the moon's influence must be almost infinitesimal, as every one can readily see, it would be difficult, perhaps, to determine its exact relation to weather changes which are so complex, but it would seem far otherwise as to the determination of the exact effect of explosions upon the atmosphere. A careful study of this question has been made by Mr Edward Powers, who has found that 158 of the smaller and larger battles of the Rebellion were followed by rain, usually twenty-four hours afterward. It might be asked, is it possible that this list comprises all the cases? While some of the battles may have been omitted, yet it seems highly probable that a diligent search must have revealed most if not all there were. It is a most remarkable fact that no mention whatever is made of the battles that were not followed by rain, and yet in an inquiry of this kind it is very essential to examine both sides of the question. During the war of the Rebellion there were over 2,200 battles, on an average probably as severe as the average of the 158 above mentioned; that is to say, about seven per cent of the battles were followed by rain. Is it at all incredible that seven per cent of these battles were followed by natural rain? In the case of the battle of Bull Run, which Mr. Powers especially picks out as a bright and shining example of his theory that explosions produce rain, it has been ascertained that there was a heavy rain in South Carolina on the first day of the battle. This rain had been previously noted farther south, and this was the rain felt at Bull Run. It would be very interesting to look up the question of how many of these 158 apparent successes were due to natural causes, but unquestionably almost all, if not all, may be ascribed to that cause. It is interesting to note that it is thought this influence may extend twenty-four hours after the explosions cease. This inference, however, is hardly tenable, for the reason that the current in which these explosions take place is borne along at the rate of 20, and, in higher strata, at 30, 40, 50, and more, miles per hour, so that the specific influence from them will be carried at least 500 miles away in twenty-four hours. If we wished to determine the effect, we would need to go to that distance from the spot where the explosions were made, and the rain that came in twenty-four hours at that spot could not by any possibility be due to the explosions.

There is only one other point to be noted here. It has been stated that while the Central Pacific Railroad was being built across the Sierra Nevada Mountains, it was necessary to explode hundreds of kegs of gunpowder every day, and this tremendous fusillade was accompanied by torrents of rain, which had never been noted before in that region, and have not been noted since. If this is a fact, it was a most remarkable phenomenon, and it would seem as though it might be established by indubitable evidence. It is a little singular that no dates or definite statements which could be verified have been given. Present rainfall reports show an abundance of rain except in two or three of the hottest months, and it seems entirely probable that persons who had been accustomed to the remarkable and long continued dryness of the plains were struck by what appeared like most abundant moisture in the mountains just at a time when there was none on the plain.

We are now prepared to investigate the value of the telegraphed result from Texas. Any one who will examine the weather maps, now sown broadcast over almost the whole country, will find that on the 11th instant there was a natural rain which extended over the whole of Texas and adjacent regions. One thing seems very evident, that absolutely no rain can be obtained out of a dry atmosphere. If the explosions can produce rain in limited quantities, yet their influence must always be exceedingly slight, and the expense of the explosions must always be all out of proportion to the amount of good done. Professor Harrington has well said that these experiments begin at the wrong end. The time may be ripe for experimenting in the atmosphere upon the cause of rain, about which we now know practically nothing. It must be conceded that until we do first experiment upon the cause of rain, all time and money used in making gross explosions will be wasted.

H. A. HAZEN.

Washington, D.C., Aug. 17.

P. S. — Since writing the above, a telegram from Midland, dated Aug. 19, states that several more preliminary explosions were made on Aug. 18, and that immediately thereafter rain began falling and continued over four hours. An examination of the weather maps for Aug. 18 has shown that the rain began, to the north of Texas, at least eleven hours before the explosions, and covered an area of over 800,000 square miles. The final tests were to be made on the 20th. H. A. H.

Experiments on Snake Locomotion.

IT is a well-known fact that a snake moves along over the ground by means of adjustable plates or scutes situated on the ventral surface of the body. How the movements of these scutes succeed each other, and what relations the different convolutions of the body bear to one another, are not so satisfactorily known. Whoever has examined the mechanism of the scutes will, I think, come to the conclusion that they must be moved by the costal muscles, and that this movement must consist in a posterior depression by which the scute offers an opposing surface to the ground. In all probability this depression is both downward and backward, thus imparting a slight forward impulse to the body. If this view of the case is correct, we would naturally expect that the act of locomotion would consist in some sort of fusion or succession of these minute individual impulses. Owing to the rapidity with which these movements are normally executed it is impossible to analyze or define their exact nature, and accordingly experiment seems to offer the only trustworthy guide to a solution of the problem. In experimenting, however, we are encountered with great difficulties at the very outset.

If we could succeed in recording the movements of an animal by means of apparatus, the construction of which was ever so delicate, can we rely on this record as a faithful expression of the natural and unimpeded movements of the animal? We can hardly feel at liberty to do so. There are at least two causes which may vitiate the results: (1) the animal is excited and annoyed by the experiments and does not act naturally; (2) the apparatus used in the experiment may directly impede the organs in the discharge of their normal functions. But while these difficulties render it impossible to obtain a record which is trustworthy in all respects, yet approximate results may be obtained which will lead up to a correct solution in the end.

In considering the locomotion of the snake, it may be well first to state what we know and what we do not know. We know that the snake generally moves on a horizontal or inclined plane, rarely elevating any part of the body to a very considerable distance above that plane. It sometimes moves with its body straightened and in a straight line, but far more frequently the body is placed so as to resemble a sinusoid, and its movements have a lateral and a direct component. The larger convolutions of the body occur in those portions which have the greatest mean diameter. The convolutions do not form simultaneously, but each travels the whole length of the body, like a wave of water, being at no two consecutive moments composed of the same parts. These waves succeed each other on opposite sides of the body, thus producing a reciprocal curve. Each wave travels from the head towards the tail, and drives its predecessor of opposite phase before it until it disappears at the tail. At times the curves do not shift to alternate sides of the body, but successive curves are formed on the same side. This motion, be it observed, is totally distinct from the reciprocal curvings described above. So much for what can be directly observed. But we cannot tell by direct observation the curves which different parts of the body would describe were they to mark the surfaces over which they move. Nor can we observe the movements of the scutes, or their correlations with the movements of the body as a whole. If we are to understand these activities, we must do so by experiment.

The following was the method of experiment employed. Short pieces of thread were run through bits of sponge saturated with ink, and these were tied around the body of the snake so that the sponges would come on the ventral surface. When these were securely tied the animal was placed on a strip of coarse paper and allowed to move. So long as the sponges were properly supplied with ink every movement made by the parts of the body thus provided was marked on the paper. Now if the different sponges were soaked with ink of different color, the simultaneous movements of different parts would be recorded, and, theoretically, with a sufficient number of sponges placed at proper intervals we would secure a complete record of all the bodily movements during a sustained period of locomotion. Such a record, however, it is impossible to obtain, for reasons which need not be mentioned.

The curves obtained by this method were by no means uniform, but varied both with the direction and velocity of the movements, and apparently with the caprice of the animal. The separate curves described by different parts of the body cannot be said to be characterized by any marked idiosyncracies. On the contrary, they appear to vary at random, now being marked by acute angles followed by beautifully rounded sinuosities, which in turn may be succeeded by protracted and irregular curves or at times figure-of-eight tracings. There is this distinction, however, between the curves described by the middle of the body and those of the distal parts. They have not so great an amplitude and are less variable. Contrary to what we would naturally expect, the synchronous curves described by different parts of the body have no discoverable agreement either in phase or in form.

From this description it might be inferred that very little of value could be derived from a study of such curves. But further study shows this inference to be ill-sustained. In interpreting the curves it is well to remember that they do not represent perfectly normal movements, because the scutes over which the sponges were tied were impeded in their action, and because rough paper is even smoother than the average ground over which the snake moves. Owing to this last circumstance the scutes would slip, and the curves would thus be shorter.

After making due allowance for the conditions which embarrass the experiments, we may perhaps still speak with some degree of confidence as to the general results, and possibly discover the existence of some fundamental laws. Perhaps the most striking fact about all the curves is, that, with very rare exceptions, they are described on opposite sides of an ideal line which may be called the axis of motion. While they demonstrate that the snake's body is capable of an almost infinite variety of movements, yet lateral movements generally prevail. There is also a tendency to consecutive repetitions, sinuosities following sinuosities, and angularities following angularities. The most irregular curves are described when the animal executes slow and hesitating movements. In this case the curves may be extended on both sides of the axis of motion, or confined to one side, when the curve is a tolerably regular succession of semicircles whose adjacent arcs form cusps. During rapid motion the sinusoid is by far the most common curve described. In fact, it may be regarded as the typical curve described by the snake's body.

It is instructive to note that when the curve assumed by the body is a sinusoid, then the curves described by different points of the body are sinusoids. The relation becomes intelligible when we reflect that the curves of the body partake of a wave-like motion, each particle vibrating, as it were, from the crest of one convolution across the axis of motion to the crest of a succeeding