finger-holes occurs, and the only example in which it projects from the side at all is from Point Barrow. Since the publication, however, another specimen comes from Cerles inlet, and this is quite puzzling. In Dr. Stolpe's paper you have my Patzcuaro specimen exactly, only mine has no ornament and is a practical every-day implement for killing ducks. The spear-shaft is ten feet long, of slender cane, and has a hole at the after end for the hook of the throwing stick. The gig consists of three iron barbs, for all the world like the Eskimo trident for water-fowl. The problem now is to connect Alaska with Patzcuaro.

O. T. MASON.

Washington, D.C., Oct. 26.

Molecular Motion in the Development of Water Waves.

WHEN waves are developed on the surface of water, whether by something thrown into or moving through the water, or by the friction of the wind blowing along the surface, the water constituting the wave rises up and sinks down, but does not move along the surface. When the friction of the wind is the cause of wave production, or when the waves are produced by any other force exerting a pull or a push in the water, there is some horizontal movement or current; but this current is not wave-motion proper, and is entirely distinct from it. The undulations in a slack rope, vibrated at one end, are true wave-motion, analogous to that which occurs on the surface of water.

If we suppose the water to consist of molecules, each having capacity for its own proper motion, and subject to the force exerted by the earth's attraction and by the pressure of other molecules above it, but free to move with comparatively small friction, the formation of waves becomes very simple. Water under the pressure when the formation of waves is possible, is incompressible, and when a solid body is thrown into or moved through the water so rapidly that the displaced particles cannot get out of the way laterally, some of them are forced up, under the well-known law that motion is in the direction of least resistance. If the body is placed in or drawn through the water slowly enough for the displaced particles to push their way horizontally, none of them are thrown up, and the initial wave is not formed. But time is required for this movement, and when the body is thrown into the water, or moved through it rapidly, the displaced particles are forced to rise up against the force of gravitation, the quantity forced up — that is, the size of the initial wave — being determined by the volume of the body and the rapidity of its movement through the water. If the force is impulsive and not constantly acting, the second wave is less than the first, and they go on diminishing until the force is expended in horizontal motion, and there is an elevation of the surface commensurate with the volume of the immersed body, — the same result precisely that would have been reached without wave-formation if the body had been immersed slowly enough.

When wind first impinges against the surface of still water, the friction pulls up a little of the water in the form of a minute initial wave, but the force being constantly acting, the wave continues to increase in size until the maximum possible from the given friction is reached.

The force of cohesion between the molecules of water is less than the pull of gravitation upon them, for if this were not the case, water would stand up like a solid mass, as ice does, instead of spreading out and flowing, in obedience to the force of gravitation, and continuing to flow until it reaches some substance in which the force of cohesion is sufficient to counteract the pull of gravitation on its molecules, or until the increased cohesion from congelation accomplishes the same result.

While the force of cohesion between the molecules of water is not sufficient to prevent them from moving in obedience to the force of gravitation, it is still considerable, and very great as compared with the force of cohesion between the molecules of air and other gases; and when a portion of the water is forced up against the force of gravitation, the substance continues in mass, and must so continue until subjected to a force sufficient to overcome both gravitation and cohesion.

When the mass lifted up in the formation of the initial wave falls back (as it must do under the constantly acting force of gravitation), with a velocity too great to be expended in horizontal motion, the molecules receiving this impact must rise up as those did which constituted the initial wave, and so on, each wave being the progenitor of that wave which follows it. If the force is impulsive, as when a body is thrown into the water, each wave is the sole progenitor of the wave following; if the force is constantly acting, like the friction of the wind, each wave in producing another is supplemented by the constantly acting force which caused the initial wave.

When the uplifted water falls back on something not so free to move as the molecules of water, — as, for instance, when the water becomes so shallow that the fall is against the bottom, or so thick with grass and water-plants as to impede the free movement of the water, — the wave-formation at once begins to diminish and soon ceases entirely. In short, the waves on the surface of the water are the result of the impact of the lifted-up mass falling back on the free to-move molecules constituting the whole mass with a velocity so great that the force cannot be transmitted horizontally.

In observing the phenomenon on a lake a few miles wide, it is interesting to note that, even in a high wind, the surface of the water near the windward shore is only a little agitated by small ripples; farther out it becomes rougher, and on the lee shore the waves have reached the highest point possible for the extent of surface and force of the wind. The pressure of the wind on the surface of an inland lake is constantly variable, even over comparatively small areas, as every one has observed who has navigated a sail-boat; and as the friction, which is the wave producing force, varies with the pressure, the waves vary in both length and height.

When the wind is high the crests of the highest waves become unable to withstand the impact of the force, and are broken into fragments or spray, forming what we call "white-caps." This phenomenon does not depend entirely on the violence of the wind, nor on the height and volume of the waves, but it depends on the relation between these two. If the waves are very large and oval (and this depends on the nature and action of the force producing them), only the most violent wind can cause white-caps, while if the waves are small but narrow and sharp, a comparatively light wind will develop them. In a portion of the water broken up in the formation of white caps, not unfrequently the force of cohesion is so far counteracted that the water is carried off in the form of spray; the residue of the white-cap not carried off as spray, instead of sinking down with the main body of the wave as in other cases, flows down the farther side of it. Hence the formation of white-caps tends to diminish rather than to increase the size of the waves.

It sometimes happens that the impact of the wind against the water elevated above the surface becomes so violent that it is all blown away as spray, and no waves are formed at all. In January, 1884, I think it was, this phenomenon occurred on Lake Eustis, in Florida. We took passage on the "Mayflower," a little side-wheel steamer of from thirty to forty tons burden, very narrow and long, and low decked, to cross the lake from east to west, the distance being about seven miles. It was blowing a breeze from the west, which caused waves probably a foot high, and sufficient to cause the little steamer to rock perceptibly. A very black cloud came up from the west, meeting us, and between one and two o'clock in the afternoon, when we were about one-fourth of the way across, a storm of wind and rain burst upon us with intense fury. Putting on my overcoat hastily, I at once made my way with difficulty through the wind and rain to the pilot-house, a little coop perched on the front end of the deck, to see that the captain, who was steering, did not lose his presence of mind, and to urge him to hold the head of the boat to the wind, from whatever direction it might come. Finding him cool and self-possessed, I returned to the cabin, another little coop amidships, and found the passengers, eight or ten in number, in great terror. Acting on a suggestion of the captain, I got out the life-preservers, and in less time than it takes me now to write this sentence, each passenger had on one, ready for the plunge which we all knew would come in a few seconds if the wind struck the boat on the side. When the life-preservers were put on, I opened my satchel and slipped my travelling flask into my overcoat pocket, leaving money and other valuables to their fate, and took position with my wife, who was one of the party, on the deck (a portion of which in rear of the little cabin was sheltered), so as not to be carried down by the boat if it capsized, and held on to a post to prevent being blown overboard. After these precautions were taken, I looked out on the lake, and to my unbounded astonishment the surface was almost perfectly smooth. The moment an incipient wave would rise above the level, the whole of it was carried away as spray by the wind. I saw this occur repeatedly. The spray and rain made it so dark that only the surface of the lake a few feet from the boat could be seen, but as far as could be seen there was violent agitation but no waves, and there was no more rocking of the boat than in a dead calm. This continued for some time, probably an hour after my first observation, when the storm abated, the clouds passed away, and the sun came out; but the wind was still blowing a stiff gale from the same direction, and soon waves from two to three feet high caused the little steamer to roll and jump more than was pleasant.

So far as I am aware, it has not been ascertained experimentally what determines the coefficient of friction of air moving over the surface of water. It is obviously this friction which causes waves in the water when the wind blows, and, like all other friction, it doubtless depends measurably on the direction and violence of the impact. But the air, by a force the operation of which is not clearly understood, and which we call evaporation, is constantly pulling out molecules of water and absorbing them in aqueous vapor.

The units of energy required to transform a pound of water into vapor is a measurable quantity, and according to the law of conservation of energy, the same units of force must be required to do this work, whether the temperature be 100° C., or 0° C., or anywhere between. The time in which the work can be done varies with the temperature, but the units of force expended must remain the same. If this is so, the force exerted in evaporation is immense, and its direction, apparently, is from the surface of the water upwards. It must therefore necessarily operate as a resistance when the air moves across the surface of the water at right angles to this direction, and thus increase friction.

It seems to be analogous to friction between two solid bodies when one of them absorbs the particles rubbed off from the other; the absorption may not increase the friction, but the rubbing-off does. So in this case, the absorption by the air of the molecules or particles of water as aquous vapor may not increase the friction between the air and the water, but pulling them away from the water certainly ought to do it.

From this it would seem that a dry wind, from its greater capacity to absorb aqueous vapor, ought to produce greater friction and higher waves than a damp wind of the same velocity. This appeared to me to be the case with the winds blowing across Lake Harris at my winter home in Florida. The dry winds following the rain-storm seem to raise higher waves than the damp winds preceding the rain and during its continuance; but without facilities or skill for accurately dotermining either the relative humidity or the velocity of the wind, such observations are of no value except to call attention to the subject.

There is another view of the matter which seems to me to be worthy of examination. If the evaporation-pull when air passes over the surface of a liquid is an element in the resulting friction and consequent wave development, we have, in the capacity of certain oils to resist evaporation, an explanation of the phenomenon that pouring oil on the surface of water diminishes the waves. This is indicated by the fact that kerosene oil, which evaporates rapidly, does not seem to have the effect of diminishing wave-formation.

Before this probable difference in friction between liquids which evaporate readily and those which resist evaporation had occurred to me I tried the experiment of pouring kerosene oil on the surface of Lake Harris when it was very rough and a high wind blowing. It had no perceptible effect in diminishing the waves, but a conscious want of skill in conducting the experiment left me in doubt as to whether the failure resulted from that or some other cause. Evaporation takes place more rapidly when the air is not moving, because fresh unsaturated portions of the atmosphere are being constantly brought into contact with the liquid surface; and the theoretical probability that this evaporation, this pulling-away of molecules of water by the air into itself, is an element in the friction between the wind and the water, is certainly sufficient to justify the labor of its experimental determination. It may be that the thorough saturation of the wind blowing across Lake Eustis, in the case above mentioned, was itself an element in preventing the wave-formation: the saturation of the wind may have diminished its friction and consequent capacity for wave development, and the blowing away of incipient waves into spray may have increased the saturation. It was obvious that the quantity of water carried off as spray was not at all comparable to that which rises above the level in waves from a wind of less violence. Taking the normal level as the average between the crest and the trough of the waves, there was much more water above that level when the lake became rough after the hurricane had passed, than appeared to be carried off in spray while no waves were being formed.

There is another element of resistance which must be taken into account in determining the friction between wind and water. The air not only absorbs water in the form of aqueous vapor, but the water holds air (or oxygen obtained from it) in solution. This is the air which the fishes breathe, and under atmospheric pressure and near the surface of the water it is estimated that the air thus held in solution constitutes about one-twentieth of the volume of the fluid. This air can be disengaged from the water. It is this disengagement of the air from the water by the suction of a pump, which renders it impossible for a pump to raise ordinary water to the full height to which the atmospheric pressure will raise a column of water from which air is excluded: when the suction of the pump exerts a pull on the water with air in solution sufficient to raise it to about twenty-seven feet, the air in the water is disengaged and fills the vacuum chamber, thus stopping the further lifting of the water by the vacuum pull. This disengagement of the air from the water goes on in water-pipes also. In a system of water-pipes the disengaged air collects in the most elevated portions of the pipes, and, unless discharged through air-valves, becomes a serious obstacle to the flow of the water.

In ice-making, this air in the water is gotten rid of by distilling the water and recondensing it, or by boiling the water and then freezing it before it has re-absorbed air. If the air is not removed in some way, it remains in the ice in small bubbles, rendering the ice white and porous.

It is certain that the agitation of water either impedes or facilitates the absorption of air into solution. The general impression is that agitating the water aerates it, that is, causes it to absorb air; but when water containing odorous vapor is stirred, they are given off. This and some other phenomena seem to me to indicate that agitation, while it enables water to retain in solution matter heavier than itself, has the opposite effect with matter lighter than itself, and that the tendency of agitation is to cause water to release gaseous matter held in solution. But whether the agitation of the water tends to cause it to take air into solution, or to release air absorbed when the water was less agitated, the process, either of absorption or of release, probably increases friction between the wind and the water, as the surface of the water becomes agitated by wave-formation. This element of friction must be very small when compared with the far greater work of evaporation, but it ought to be taken into consideration in determining the difference in friction between air and water and air and oil.

It has been demonstrated, experimentally, that when water evaporates into air as aqueous vapor the process goes on by molecule after molecule, and not by aggregations of molecules or masses; nor that water absorbs oxygen from the air by taking in each molecule separately; but, according to the accepted theory of diffusion of gases, we must assume that the aqueous vapor resulting from evaporation diffuses into the atmosphere by molecules and not by masses; and the fact that the oxygen of the air dissolved in water is separated from the nitrogen, the molecular constitution of the matter absorbed being different from what it was before its solution, leaves no doubt that that process is molecular also: the oxygen and nitrogen molecules, whose intermixture, through diffusion, constitute the atmosphere, are disassociated, the water taking into solution a much larger proportion of the oxygen. This could not possibly occur if the process of solution were not molecular. If the air is composed of the molecules of oxygen and molecules of nitrogen so intermixed as to constitute a continuous substance, a process which takes more of the oxygen than it does of the nitrogen is necessarily molecular.

It seems, therefore, that we are authorized to conclude not only that the waves themselves are the result of motion of the molecules constituting the water, and not of masses of such molecules, but that when wind causes the waves, its friction, in part if not entirely, is due to the passage of molecules from one fluid into the other. DANIEL S. TROY.

Montgomery, Ala , Oct. 23.

Rain-Making.

AS Professor Hazen, in his letter published in *Science* of Oct. 16, garbles the quotation from Plutarch which is relied on to prove that the ancients had the same notion in regard to rains following battles that prevails at the present time, I beg leave to give the passage entire, for it is only by a consideration of the whole that his meaning can be arrived at. Plutarch says, in his life of Marius, speaking of the defeat of the Ambrones by the Romans:

'The Romans pursuing, either killed or took prisoners above a hundred thousand. Other historians give a different account of the number of the slain. From these writers we learn that the Massilians walled in their vineyards with the bones they found in the field, and that the rain which fell the winter following, soaking in the moisture of the putrefied bodies, the ground was so enriched by it that it produced the next season a prodigious crop. It is to be observed, indeed, that extraordinary rains generally follow after great battles; whether it be that some deity chooses to wash and purify the earth with water from above, or whether the blood and corruption, by the moist and heavy vapors they emit, thicken the air, which is liable to be altered by the smallest cause."

Now, if we take by itself the statement that "extraordinary rains generally follow after great battles," it would appear, indeed, that the ancient ideas on this subject were identical with those prevailing in modern times. But if we ask the question, "How long after the battles did the rains occur to which Plutarch alluded?" and look for our answer in the context, we shall see, as I said in my letter in *Science* of Oct. 7, that the notions of the former on the subject appear to have been wholly different from those of the latter. When did the rains follow the battle between the Ambrones and the Romans? In the winter following. When did rains follow any other battles that Plutarch had in mind, or when did he think they followed? After the bodies of the dead had putrefied. How soon could the "blood and corruption" — especially the corruption — emit "moist and heavy vapors?" Not under a week. How soon could "some deity wash and purify the earth with water from above?" Not under several months.

It matters not how erroneous Plutarch's ideas were as to why rains followed after battles. It is not his conclusions with which we have to deal, but we are trying to find out what he supposed the facts to be on which he based them. In doing this we have no right to assume as facts anything that is inconsistent with his view of the case.

Professor Hazen quotes the opinion of another rain-maker in opposition to my own. He might also have quoted me against myself. In an article written by me and published in the *Golden Age* of May 11, 1872, and which is also copied into the appendix to the revised edition of "War and the Weather," occurs the following passage:

"If great noises cause rain, some other less expensive way may be devised to produce them. It was noticed, even in ancient times, that great rains followed battles, and it is not impossible that the shouts of a great multitude, with the clashing of metal on metal, may produce the same effect upon the air as the firing of cannon. Should all the inhabitants of a city at a given hour unite in creating an uproar with hands and voices, it would seem to one in our day as though the world were returning to barbarism; but in the higher civilization of some age to come, this may perhaps be a common occurrence."

The other rain-maker referred to has evidently adopted this idea without having made any more critical examination of the passage quoted from Plutarch than I had done when the above was written. But though I have changed my mind in regard to the meaning of this passage, it would be going too far to say that ancient battles did not immediately produce rain, and that the above does not furnish the true explanation of the phenomenon. I only affirm that Plutarch did not say that rains immediately followed great battles, and that the inference that he thought they did cannot be drawn from what he does say. I contend further that, even if the ancients thought that battles produced rain, they may have been wrong, while the moderns may be right in that opinion. Coincidences sometimes occur in thought as well as in action and events.

In speaking of the battles of the late war, and their supposed effect upon the atmosphere, Professor Hazen says, "Mr. Powers thinks that the currents of the atmosphere do not travel at the rate of twenty to fifty miles per hour, or, at least, during these battles they did not do so." This is hardly a fair statement of my position. I think it very probable that portions of two currents moving in nearly opposite directions, in mingling together, lose to a great extent their original motion, and take on a circular motion, moving for a time neither very far east nor very far west. I think that in this way the influence of the concussions may remain in the vicinity of the firing until enough air of different temperatures has mixed together to develop a rain-storm, and that then the storm will move eastward along with the current that supplies the greater portion of the moisture that forms the rain.

Professor Hazen repeats his statement that "one thing seems very certain, that absolutely no rain can be obtained out of a dry atmosphere," and eliminates from it the word "seems." It is not apparent how this helps it as an argument against the artificial-rain theory. According to my understanding of his first article, he did not state this as an abstract idea, but in order to show how unreasonable it was, in his view, to expect to produce rain by concussion in certain states of the atmosphere; and by "atmosphere" I naturally understood him to mean the same thing that he would mean if he were speaking of measuring the humidity of the atmosphere with his instruments. My contention is that there is nothing unreasonable in expecting to produce rain, however dry such air may be, for we are constantly receiving, by the vehicle of air-currents, supplies of aqueous vapor from the tropical portion of the Pacific Ocean; and these currents and the vapor they bring occupy a high altitude, and there the clouds and rain are formed.

Professor Hazen says, "It certainly is not a fact that two currents pass in opposite directions near the point of formation of our storms." How does he know this? He must admit that there is a current moving constantly from west to east or from south-west to north-east. How does he know what there is above this current? Professor Maury gives very strong reasons for believing that there is a polar current there flowing in nearly the opposite direction. Has any one ever given as good reasons for believing to the contrary? Professor Maury's theory was not evolved from a few isolated facts, but from a comprehensive knowledge of the winds throughout the whole world, or so much of it as could be reached by navigators. Has his theory of the circulation of the atmosphere ever been overturned, or even seriously attacked? When I speak of air-currents, one bringing tropical moisture and the other polar cold, I am not drawing upon my own imagination for props to support the theory of artificial rain production, but I am availing myself of the result of investigations and deductions by one who, as a man of science, was a peer to any whom this country has ever produced.

Delavan, Wis., Oct. 19. EDWARD POWERS.