Correspondence.

The Philosophy of Vision.

To the Editor of the Scientific American:

In your issue of January 16, in answer to the query, "What makes the sun and moon appear so much larger when in the horizon than when high up in the sky?" you remark that the atmosphere then acts as a lens. As I have always taken great interest in anything relating to the most wonderful of all the senses—that of sight—I will, with your permission, give some reasons in your valuable journal for thinking that is not the reason for their enlarged appearance. If they are magnified by the air at the horizon, then they will subtend when seen at that point a larger angle when measured by a theodolite or any instrument adapted to measuring angles. In the absence of costly instruments, a simple one can be made to show that the enlargement is wholly deceptive and not a real magnification. Take a flat stick like a ruler, some two feet long or more, and at one end fasten a piece of tin or card with a small orifice, say one-eighth inch, placed so as to look along the length of the rule; then, about two feet from this peek-hole, place two pins in the wood, just far enough apart to take in the moon at the horizon when the eye is at the peek-hole. They will be a little less than one-half inch apart if two feet from the peek-hole, for the apparent diameter of both sun and moon is about one-half degree.

Now, with the same apparatus, look at the moon when high in the heavens, and it will be found just as large as at the horizon. Why, then, the great apparent difference? To answer this we must consider some of the laws and principles of this wonderful sense of vision. First, we judge *primarily* of the size of objects by the angle of vision they subtend; but with a given object, experience soon shows us that this angle varies inversely as the distance, and we learn to make allowance for distance when judging of size. Thus the tip of my little finger, when held arm's length from my eye, covers up entirely a large dwelling house about one mile distant; but as I look at the house it appears full size, and I should never think of calling it as small as my finger tip. Why? Because I learned to unconsciously allow for its distance. The same principle is illustrated in the answers given to the question, "How large does the moon look to you?" The answers vary from a tea saucer to a cart wheel; but the tip of the little finger when held farthest from the eye will more than hide the moon, even at the horizon. Yet no one thinks of comparing fair Luna to so small an object as the tip of their smallest digit.

It follows from all these facts that our judgment of the size of objects depends very much upon the allowance we unconsciously make for distance. The hunter anxiously looking for game may vainly fire away at a supposed coon in a tree, but, failing to bring him down, may find that a minute brown insect hanging by its web in front of his eye, but supposed to be at a distance, was mistaken for a much larger animal and led to a vain expenditure of ammunition. This same principle is well illustrated when exhibiting the planets to unaccustomed observers through the telescope. I have often, when exhibiting Jupiter with a power that would magnify it to twice the moon's diameter, therefore to four times its size, inquired of the observer, "How large does it appear to you? As large as the moon?" "Oh no, not half! Why about the size of a dollar." Then I would say, "Open your other eye to take in more of the heavens, and remember you are not looking into the telescope, but through it at the sky;" and they would instantly be convinced that it appeared much larger than the full moon. I was once walking along a street with which I was well acquainted, and saw upon one of the neighboring heights a building with a cupola of large cylindrical form that I had never noticed before, although I had often seen, as I supposed, every structure in that neighborhood. Looking more carefully and changing my position a little, the supposed cupola instantly shrunk into an electric street lamp with its metal protecting cylinder, which the supposition of distance had magnified into a large cupola. But to return to our muttons: the moon when in the horizon is seen to be beyond the houses, the trees, the hills, and even the huge mountains, and, we unconsciously allowing for her great distance, she swells out to an immensely greater size than when seen high in the heavens, where we have no such convincing proof of her distance. If these views are correct, the phenomenon is a subjective and not an objective one, or, to put it in more elegant Greek, it is all in your eye. R. S. BOSWORTH.

Brownville, Feb. 19, 1892.

A Problem in Physics.

To the Editor of the Scientific American

Consider a cylinder one square foot in area and two feet high, with a piston having no weight, but moving air tight in the cylinder. Suppose that no heat can escape from the cylinder, and that the two cubic feet of air are at atmospheric pressure and a temperature of 60° If we place weights upon the piston, one pound at a time, until the total amounts to 2,160 pounds, the piston will continue to descend till it has fallen not quite one foot. The air in the cylinder will be compressed to not quite two atmospheres, and will have a temperature, according to the formula in *Science*, February 19, of about 169°. The work performed will be equivalent to the fall of 1,080 pounds one foot, and a little calculation will show that this amount of work would raise the temperature almost exactly to that given by the formula. It is easy to see that all the heat due to the work of compression in this case will be concentrated upon the air, if we consider that neither the piston nor the cylinder absorbs any of the heat. If now we should remove the weights one by one, it is easy to see that the air in expanding would gradually lift the weight on the piston, and as the last pound was removed the air would return to atmospheric pressure and to a temperature of 60°.

Suppose, now, that when the piston had descended one foot we had cooled off the compressed air until it had reached 60°, then upon expanding, as in the last instance, the air would be cooled to about 29° below zero, as shown by the formula. This discussion seems to be perfectly plain thus far, but now we come to a point that is not quite so easy to elucidate.

Joule, in experimenting upon a practical determination of the mechanical equivalent of heat, was obliged to immerse his cylinder, into which the air was to be compressed, together with the compressing pump, in a vessel of water, in order that all the heat developed by the work of compression should enter the water bath. We then can state at once the proposition: *If air when compressed is to be raised to the temperature indicated by theory, it is very essential that all the heat developed in the work of compression, ignoring that due to friction, should enter the air.* This seems a self-evident proposition, but, plain as it is, it is a fact that nearly all the errors which have arisen in the various discussions bearing on this question have come from a neglect of this very obvious statement. In this experiment of Joule's, let us suppose that the compression pump had been in one water bath and the cylinder in another. Under these conditions the first bath would have received the more heat, supposing that the air when compressed lost none of its heat in passing from the pump to the cylinder. If now the air, in passing from the pump to the cylinder, were cooled to the air temperature, nearly all the heat due to the work of compression would have been either retained in the bath around the pump, or would have been lost on the passage to the cylinder, and, in consequence, the air in the cylinder would have been heated only a very little in being crowded to two atmospheres.

Instead of connecting the pump directly with the cylinder, let us take two cylinders of equal size having a connecting tube closed at first. Let us compress the air in one cylinder to three atmospheres, the air in the other being at atmospheric pressure. The air will be heated by the compression to about 245°. Now, on connecting the two cylinders, an equilibrium will very quickly be established, the air in the first cylinder will be very slightly cooled in imparting a velocity to its particles, but will still remain at not far from 230 or 240°. The air in the other cylinder will be very slightly heated by the impact of the air rushing in, and will be heated much more by the hot air from the first cylinder, the resulting temperature being not far from 160°. But now, if the air had been cooled in the first cylinder to the air temperature before the connection was made, it is very plain that the cooling in one and the heating in the other would have been exceedingly slight.

In some experiments by the present writer the air was cooled after leaving the compressing pump and before it entered the cylinder, and the resulting temperature, after compressing to 1⅓ its former pressure, was about 4° higher than before. If in this case all the heat due to the work of compression had entered the cylinder, the temperature of the air would have been raised 43°. It was noticed always that the compressing pump became very highly heated during the operation. We may say, then, that the mere crowding together of the particles of air does not develop any heat. Joule found that when he had one cubic foot of air at two atmospheres heated to a certain temperature, they were still at that temperature when expanded into two cubic feet.

There is one other phase of this problem. Suppose that, instead of opening the cylinder in which the air was compressed to three atmospheres into another cylinder in which the air was at atmospheric pressure, we had opened it to the free air. In this case the resistance to the flow of the air would have been much less than before, the velocity imparted to the particles of air would have been somewhat greater than before, and the resulting cooling, due to the work of giving this velocity, slightly greater; but it would have been exceedingly slight as compared with the cooling which would have resulted had the gas expanded against a resistance. The present writer, under the latter conditions, found that, when the compression had been carried to 1⅓ times atmospheric pressure, the cooling on releasing the pressure was about 4° and not 38°, as would have been the case if the gas had expanded against a resistance.

The most important point dwelt on here is the fact that a mere crowding together of air particles does not develop heat. It is with some diffidence, and only after the most careful and long continued study of the problem, that I have brought my mind to accept this proposition. I most sincerely trust that physicists will consider this question and present their views upon it. It seems to me of the profoundest importance in many theories lying at the base of the science of meteorology, and an elucidation of this fact may lead to important discoveries in that science. I am well aware of the fact that the mind of the physicist recoils from the thought that there can be any expenditure of energy in compressing a gas without the latter becoming enormously heated; but there seems no way of avoiding the conclusion that sometimes the heat developed in the work of compression may be dissipated while the particles of gas themselves are crowded together.

H. A. HAZEN.

Feb. 22, 1892.

Montana Sapphires.

What with emeralds, hiddenite, and rutile from North Carolina; topaz, phenacite, and aquamarine from Colorado; garnets and peridots from Arizona; opals from Oregon and Idaho; thompsonites, chlorastrolites, and amethysts from the Lake Superior region; tourmalines from Maine; golden beryl from Connecticut; pearls from Wisconsin, Tennessee, and the Pacific coast; sphenes and diopsides from New York; turquoise from New Mexico; agate and onyx from the Rocky Mountain belt; rock crystal and smoky quartz from the Alleghanies and Arkansas; and sapphires from Montana, it seems as if the United States had become one of the principal gem-producing countries of the world. Unfortunately, for some reasons, the sapphires of Montana have slipped through the fingers of the people who should own them, and are now mined by an English syndicate, that has paid in $2,000,000, the best stones being sent to London, where high prices are demanded for them.

The diggings are known as the Spratt sapphire ground, and are about twelve miles north of Helena, on the Missouri River. A fact that is not generally known is that the soil is rich in gold as well as gems, and that two assays from the tailings have shown $58 and $71 to the ton respectively. There are three important bars—El Dorado, Ruby, and French—where the stones are found, and such is their abundance that 1,016 of them were "jigged out" of two wheelbarrow loads of gravel from El Dorado bar in a few minutes. The material of the bars appears to be glacial drift, varying in depth from 30 inches to 30 feet, but also includes rock that seems to have been broken down from a dyke a thousand feet high and nine miles up the river. Gold, silver, and galena are found in the neighborhood, and it is whispered that another deposit of sapphires was recently found, and that the land is being quietly bought up by American miners, but the country immediately about is a grazing land, encircled by mountains. Nodules of limonite—round, oval, lenticular, and reniform in shape, with mamillary markings—are of common occurrence in the bars.

The Kleinsmith collection of gold specimens found along the Missouri at this point, and on view at the national bank at Helena, numbers 500 gold crystals, for which $4,500 has been refused. The stones grade from almost water-white to sky-blue—none have been found of the deep blue color shown by Oriental sapphires—and incidentally exhibit green, lavender, pink, and gray. A few show dichroism, green and blue being discovered in alternation, and in a few cases blue and red. Stellation and chatoyancy are not uncommon, but perfect stars have not been discovered. One interesting stone cut with facets shows a series of concentric "phantoms" that are revealed in milky lines when the gem is held with the table before the eye. These stellations and phantoms occasionally make the interior of the stone appear turbid, but there are several gems of good size, three or four carats in weight, that are magnificent in brilliancy.—*Minerals.*

Pineapple Juice.

Some time ago the late Dr. V. Marcano, of Venezuela, noted that pineapple juice contained a proteid-digesting substance. No careful study of this fact was, however, made by him. Recently, Professor R. H. Chittenden, assisted by Messrs. E. P. Joslin and F. S. Meara, have investigated the matter fully, and announce facts which are likely to give to the succulent pineapple a prominent place in dietetics.

Pineapple juice is an acid fluid of specific gravity of 1·043. An ordinary pineapple yields 600 to 800 cubic centimeters of it. The proteid-digesting power is quite remarkable in its intensity. Three ounces of the juice will dissolve ten or fifteen grains of dried albumen in four hours. The action takes place in acid, neutral, or even alkaline media, thus resembling trypsin more than pepsin. It acts best in neutral solutions. The pineapple juice contains also a milk-curdling ferment. A well-known meat powder is said to be prepared with the help of pineapple juice.—*Med. Record.*