I have diagrammatically shown the amount of different colors which penetrated on the same days, taking the Riffel as ten. It will be seen that on December 23 we have really very little violet and less than half the green, although we have four fifths of the red.

The next diagram before you shows the minimum loss of light which I have observed for different air thicknesses. On the top we have the calculated intensities of the different rays outside our atmosphere. Thus we have that through one atmosphere, and two, three, and four. And you will see what enormous absorption there is in the blue end at four atmospheres. The areas of these curves, which give the total luminosity of the light, are 761, 662, 577, 503, and 439; and if observed as astronomers observe the absorption of light, by means of stellar observations, they would have had the values, 761, 664, 578, 504, and 439—a very close approximation one to the other.

Next notice in the diagram that the top of the curve gradually inclines to go to the red end of the spectrum as you get the light transmitted through more and more air, and I should like to show you that this is the case in a laboratory experiment. Taking a slide with a wide and long slot in it, a portion is occupied by a right angled prism, one of the angles of 45° being toward the center of the slot. By sliding this prism in front of the spectrum I can deflect outward any portion of the spectrum I like, and by a mirror can reflect it through a second lens, forming a patch of light on the screen overlapping the patch of light formed by the undeflected rays. If the two patches be exactly equal, white light is formed. Now, by placing a rod as before in front of the patch, I have two colored stripes in a white field, and though the background remains of the same intensity of white, the intensities of the two stripes can be altered by moving the right angled prism through the spectrum. The two stripes are now apparently equally luminous, and I see the point of equality is where the edge of the right angled prism is in the green. Placing a narrow cell filled with our turbid medium in front of the slit, I find that the equality is disturbed, and I have to allow more of the yellow to come into the patch formed by the blue end of the spectrum, and consequently less of it in the red end. I again establish equality. Placing a thicker cell in front, equality is again disturbed, and I have to have less yellow still in the red half, and more in the blue half. I now remove the cell, and the inequality of luminosity is still more glaring. This shows, then, that the rays of maximum luminosity must travel toward the red as the thickness of the turbid medium is increased.

The observations at 8,000 feet, here recorded, were taken on September 15, at noon, and of course in latitude 46° the sun could not be overhead, but had to traverse what would be almost exactly equivalent to the atmosphere at sea level. It is much nearer the calculated intensity for no atmosphere intervening than it is for one atmosphere. The explanation of this is easy. The air is denser at sea level than at 8,000 feet up, and the lower stratum is more likely to hold small water particles or dust in suspension than is the higher.

For, however small the particles may be, they will have a greater tendency to sink in a rare air than in a denser one, and less water vapor can be held per cubic foot.

FIG. 3.—PROPORTIONS OF TRANSMITTED COLORS.

Looking, then, from my laboratory at South Kensington, we have to look through a proportionately larger quantity of suspended particles than we have at a high altitude when the air thicknesses are the same. And consequently the absorption is proportionately greater at sea level than at 8,000 feet high. This leads us to the fact that the real intensity of illumination of the different rays outside the atmosphere is greater than it is calculated from observations near sea level. Prof. Langley, in this theater, in a remarkable and interesting lecture, in which he described his journey up Mount Whitney to about 12,000 feet, told us that the sun was really blue outside our atmosphere, and at first blush the amount of extra blue which he deduced to be present in it would, he thought, make it so. But though he surmised the result from experiments made with rotating disks of colored paper, he did not, I think, try the method of using pure colors, and consequently, I believe, slightly exaggerated the blueness which would result.

I have taken Prof. Langley's calculations of the increase of intensity for the different rays, which I may say do not quite agree with mine, and I have prepared a mask which I can place in the spectrum, giving the different proportions of each ray as calculated by him, and this when placed in front of the spectrum will show you that the real color of sunlight outside the atmosphere, as calculated by Langley, can scarcely be called bluish. Alongside I place a patch of light which is very closely the color of sunlight on a July day at noon in England. This comparison will enable you to gauge the blueness, and you will see that it is not very blue, and, in fact, not bluer perceptibly than that we have at the Riffel, the color of the sunlight at which place I show in a similar way. I have also prepared some screens to show you the value of sunlight after passing through five and ten atmospheres. On an ordinary clear day you will see what a yellowness there is in the color. It seems that after a certain amount of blue is present in white light, the addition of more makes but little difference in the tint. But these last patches show that the light which passes

through the atmosphere when it is feebly charged with particles does not induce the red of the sun as seen through a fog. It only requires more suspended particles in any thickness to induce it.

In observations made at the Riffel, and at 14,000 feet, I have found that it is possible to see far into the ultra-violet, and to distinguish and measure lines in the sun's spectrum which can ordinarily only be seen by the aid of a fluorescent eye piece or by means of photography. Circumstantial evidence tends to show that the burning of the skin, which always takes place in these high altitudes in sunlight, is due to the great increase in the ultra-violet rays. It may be remarked that the same kind of burning is effected by the electric arc light, which is known to be very rich in these rays.

Again, to use a homely phrase, "You cannot eat your cake and have it." You cannot have a large quantity of blue rays present in your direct sunlight and have a luminous blue sky. The latter must always be light scattered from the former. Now, in the high Alps you have, on clear day, a deep blue-black sky, very different indeed from the blue sky of Italy or of England; and as it is the sky which is the chief agent in lighting up the shadows, not only in those regions do we have dark shadows on account of no intervening—what I will call—mist, but because the sky itself is so little luminous. In an artistic point of view this is important. The warmth of an English landscape in sunlight is due to the highest lights being yellowish, and to the shadows being bluish from the sky light illuminating them. In the high Alps the high lights are colder, being bluer, and the shadows are dark, and chiefly illuminated by reflected direct sunlight. Those who have traveled abroad will know what the effect is. A painting in the Alps, at any high elevation, is rarely pleasing, although it may be true to nature. It looks cold, and somewhat harsh and blue.

In London we are often favored with easterly winds, and these, unpleasant in other ways, are also destructive of that portion of the sunlight which is the most chemically active on living organisms. The sunlight composition of a July day may, by the prevalence of an easterly wind, be reduced to that of a November day, as I have proved by actual measurement. In this case it is not the water particles which act as scatterers, but the carbon particles from the smoke.

Knowing, then, the cause of the change in the color of sunlight, we can make an artificial sunset, in which we have an imitation light passing through increasing thicknesses of air largely charged with water particles. [The image of a circular diaphragm placed in front of the electric light was thrown on the screen in imitation of the sun, and a cell containing hyposulphite of soda placed in the beam. Hydrochloric acid was then added; as the fine particles of sulphur were formed, the disk of light assumed a yellow tint, and as the decomposition of the hyposulphite progressed, it assumed an orange and finally a deep red tint.] With this experiment I terminate my lecture, hoping that in some degree I have answered the question I propounded at the outset—why the sun is red when seen through a fog.

THE WAVE THEORY OF SOUND CONSIDERED.

By HENRY A. MOTT, Ph.D., LL.D.

BEFORE presenting any of the numerous difficulties in the way of accepting the wave theory of sound as correct, it will be best to briefly represent its teachings, so that the reader will see that the writer is perfectly familiar with the same.

The wave theory of sound starts off with the assumption that the atmosphere is composed of molecules, and that these supposed molecules are free to vibrate when acted upon by a vibrating body. When a tuning fork, for example, is caused to vibrate, it is assumed that the supposed molecules in front of the advancing fork are crowded closely together, thus forming a condensation, and on the retreat of the fork are separated more widely apart, thus forming a rarefaction. On account of the crowding of the molecules together to form the condensation, the air is supposed to become more dense and of a higher temperature, while in the rarefaction the air is supposed to become less dense and of lower temperature; but the heat of the condensation is supposed to just satisfy the cold of the rarefaction, in consequence of which the average temperature of the air remains unchanged.

The supposed increase of temperature in the condensation is supposed to facilitate the transference of the sound pulse, in consequence of which, sound is able to travel at the rate of 1,095 feet a second at 0° C., which it would not do if there was no heat generated.

In other words, the supposed increase of temperature is supposed to add $\frac{1}{2}$ to the velocity of sound.

If the tuning fork be a Koenig C³ fork, which makes 256 full vibrations in one second, then there will be 256 sound waves in one second of a length of $\frac{1}{256}$ or 4.23 feet, so that at the end of a second of time from the commencement of the vibration, the foremost wave would have reached a distance of 1,095 feet, at 0° C.

The motion of a sound wave must not, however, be confounded with the motion of the molecules which at any moment form the wave; for during its passage every molecule concerned in its transference makes only a small excursion to and fro, the length of the excursion being the amplitude of vibration, on which the intensity of the sound depends.

Taking the same tuning fork mentioned above, the molecule would take $\frac{1}{256}$ of a second to make a full vibration, which is the length of time it takes for the pulse to travel the length of the sound wave.

For different intensities, the amplitude of vibration of the molecule is roughly $\frac{1}{16}$ to $\frac{1}{1000000}$ of an inch. That is to say, in the case of the same tuning fork, the molecules it causes to vibrate must either travel a distance of $\frac{1}{16}$ or $\frac{1}{1000000}$ of an inch forward and back in the $\frac{1}{256}$ of a second or in one direction in the $\frac{1}{512}$ of a second.

I might further state that the pitch of the sound depends on the number of vibrations and the intensity, as already indicated by the amplitude of stroke—the timbre or quality of the sound depending upon factors which will be clearly set forth as we advance.

Having now clearly and correctly represented the wave theory of sound, without touching the physiological effect perceived by means of the ear, we will proceed to consider it.

We must first consider the state in which the sup-

posed molecules exist in the air, before making progress.

The present science teaches that the diameter of the supposed molecules of the air is about $\frac{1}{1000000}$ of an inch (Tait); that the distance between the molecules is about $\frac{1}{100000}$ of an inch; that the velocity of the molecules is about 1,512 feet a second at 0° C., in its free path; that the number of molecules in a cubic inch at 0° C. is 3,505,519,800,000,000 or 35 followed by 17 ciphers (35)¹⁷; and that the number of collisions per second that the molecules make is, according to Boltzmann, for hydrogen, 17,700,000,000, that is to say, a hydrogen molecule in one second has its course wholly changed over seventeen billion times. Assuming seventeen billion or million to be right for the supposed air molecules, we have a very interesting problem to consider.

The wave theory of sound requires, if we expect to hear sound by means of a C³ fork of 256 vibrations, that the molecules of the air composing the sound wave must not be interfered with in such a way as to prevent them from traveling a distance of at least $\frac{1}{100000}$ of an inch forward and back in the $\frac{1}{256}$ of a second. The problem we have to explain is, how a molecule traveling at the rate of 1,512 feet a second through a mean path of $\frac{1}{100000}$ of an inch, and colliding seventeen billion or million times a second, can, by the vibration of the C³ fork, be made to vibrate so as to have a pendulous motion for $\frac{1}{256}$ of a second and vibrate through a distance of $\frac{1}{16}$ to the $\frac{1}{1000000}$ of an inch without being changed or mar its harmonic motion.

It is claimed that the range of sound lies between 16 vibrations and 30,000 (about); in such extreme cases the molecules would require $\frac{1}{16}$ and $\frac{1}{30000}$ of a second to perform the same journey.

It must not be forgotten that a mass moving through a given distance has the power of doing work, and the amount of energy it will exercise will depend on its velocity. Now, a molecule of oxygen or nitrogen, according to modern science, is a mass $\frac{1}{1000000}$ of an inch in diameter, and an oxygen molecule has been calculated to weigh 0.000000054044 ounce. Taking this weight traveling with a velocity of 1,512 feet a second through an average distance of $\frac{1}{100000}$ of an inch, the battering power or momentum it would have can be shown to be in round numbers capable of moving $\frac{1}{100000}$ of an ounce.

Now, when the C³ tuning fork has been vibrating for some time, but still sounding audibly, Prof. Carter determined that its amplitude of stroke was only the $\frac{1}{17000}$ of an inch, or its velocity of motion was at the rate of $\frac{1}{33}$ of an inch in one second, or one inch in 33 seconds (over half a minute), or less than one foot in one hour.

Assuming one prong to weigh two ounces, we have a two-ounce mass moving $\frac{1}{17000}$ of an inch with a velocity of $\frac{1}{33}$ of an inch in one second. The prong, then, has a momentum or can exercise an amount of energy equivalent to $\frac{1}{33}$ of an ounce, or can overcome the momentum of 1,000 molecules.

It would be difficult to discover not only how a locust can expend sufficient energy to impart to molecules of the air, so as to set them in a forced vibration, and thus enable a pulse of the energy imparted to control the motion of the supposed molecules of the air for a mile in all directions, but also to estimate the amount of energy the locust must expend.

According to the wave theory, a condensation and rarefaction are necessary to constitute a sound wave. Surely, if a condensation is not produced, there can be no sound wave! We have then no need to consider anything but the condensation or compression of the supposed air molecules, which will shorten the discussion. The property of mobility of the air and fluidity of water are well known. In the case of water, which is almost incompressible, this property is well marked, and unquestionably would be very nearly the same if water were wholly incompressible. In the case of the air, it is conceded by Tyndall, Thomson, Daniell, Helmholtz, and others that any compression or condensation of the air must be well marked or defined to secure the transmission of a sound pulse. The reason for this is on account of this very property of mobility. Tyndall says: "The prong of the fork in its swift advancement condenses the air." Thomson says: "If I move my hand vehemently through the air, I produce a condensation." Helmholtz says: "The pendulum swings from right to left with a uniform motion. Near to either end of its path it moves slowly, and in the middle fast. Among sonorous bodies which move in the same way, only very much faster, we may mention tuning forks." Tyndall says again: "When a common pendulum oscillates, it tends to form a condensation in front and a rarefaction behind. But it is only a tendency; the motion is so slow, and the air so elastic, that it moves away in front before it is sensibly condensed, and fills the space behind before it can become sensibly dilated. Hence waves or pulses are not generated by the pendulum." And finally, Daniell says: "A vibrating body, before it can act as a sounding body, must produce alternate compressions and rarefactions in the air, and these must be well marked. If, however, the vibrating body be so small that at each oscillation the surrounding air has time to flow round it, there is at every oscillation a local rearrangement—a local flow and reflow of the air; but the air at a distance is almost wholly unaffected by this."

Now, as Prof. Carter has shown by experiment that a tuning fork while still sounding had only an amplitude of swing of $\frac{1}{17000}$ of an inch, and only traveled an aggregate distance of $\frac{1}{33}$ of an inch in one second, or one inch in 33 seconds, surely such a motion is neither "swift," "fast," nor "vehement," and is unquestionably much "slower" than the motion of a pendulum. We have only to consider one forward motion of the prong, and if that motion cannot condense the air, then no wave can be produced; for after a prong has advanced and stopped moving (no matter for how short a time), if it has not compressed the air, its return motion (on the same side) cannot do anything toward making a compression. If one such motion of $\frac{1}{17000}$ of an inch in $\frac{1}{512}$ of a second cannot compress the air, then the remaining motions cannot. There is unquestionably a "union limit" between mobility and compressibility, and unless this limit is passed, mobility holds sway and prevents condensation or compression of the air; but when this limit is passed by the exercise of sufficient energy, then compression of the air results. Just imagine the finger to be moved through the air at a velocity of one foot in one hour; is it possible that any scientist who considers the problem in connection

with the mobility of the air, could risk his reputation by saying that the air would be compressed? Heretofore it was supposed that a prong of a tuning fork was traveling *fast* because it vibrated so many times in a second, never stopping to think that its velocity of motion was entirely dependent upon the distance it traveled. At the start the prong travels $\frac{1}{8}$ of an inch, but in a short time, *while still sounding*, the distance is reduced to $\frac{1}{17,100}$ of an inch. While the first motion was quite fast, about 25 inches in a second, the last motion was only about $\frac{1}{8}$ of an inch in the same time, and is consequently 825 times slower motion. The momentum of the prong, the amount of work it can do, is likewise proportionately reduced.

Some seem to imagine, without thinking, that the elasticity of the air can add additional energy. This is perfectly erroneous; for elasticity is a mere property, which permits a body to be compressed on the application of a force, and to be dilated by the exercise of the force stored up in it by the compression. No property of the air can impart any energy. If the momentum of a molecule or a series of molecules extending in all directions for a mile is to be overcome so as to control the character of the movements of the molecules, then sufficient *external* energy must be applied to accomplish the task; and when we think that one cubic inch of air contains 3,505,519,800,000,000 molecules, to say nothing about the number in a cubic mile, which a locust can transmit sound through, we are naturally compelled to stop and think whether the vibrations of *supposed* molecules have anything or can have anything to do with the transference of sound through the air.

If control was only had of the distance the vibrating molecule travels from its start to the end of its journey, then only the intensity of the sound would be under subjection; but if at every *infinitesimal instant* control was had of its amplitude of swing, then the character, timbre, or quality of the sound is under subjection. It is evident, then, that the blows normally given by one molecule to another in their supposed constant bombardment must not be sufficient to alter the character of vibration a molecule set in oscillation by a sounding body must maintain, to preserve the timbre or quality of the sound in process of transmission; for if any such alteration should take place, then, naturally, while the pitch, and perhaps intensity, might be transmitted, the quality of the sound would be destroyed.

Again, it is certain that no molecule can perform two sets of vibrations, two separate movements, at the same time, any more than it can be in two places at the same time.

When a band of music is playing, the molecule is supposed to make a complex vibration, a resultant motion of all acting influences, which the ear is supposed to analyze. It remains for the mathematician to show how a molecule influenced by twenty or more degrees of applied energy, and twenty or more required number of frequencies of vibration at the same time, can establish a resultant motion which will transmit the required pitch, intensity, and timbre of each instrument.

When a molecule is acted on by various forces, a resultant motion is unquestionably produced, but this would only tend to send the molecule forward and back in *one* direction, and, in fact, a direction it might have taken in the first place if hit properly.

How any resultant can be established as regards the time necessary for the molecule to take so as to complete a full vibration for the note C₁, which requires $\frac{1}{16}$ of a second, and for other notes up to C₈, which only requires $\frac{1}{16,777,216}$ of a second, as when an orchestra is playing, is certainly beyond human comprehension, if it is not beyond the "transcendental mathematics" of the present day.

Unquestionably, the able mathematicians Lord Rayleigh, Stokes, or Maxwell, if the problem was submitted to them, would start directly to work, and deduce by so called "higher mathematics" the required motions the molecules would have to undergo to accomplish this marvelous task—the same as they have established the diameter of the *supposed* molecules, their velocity, distance apart, and number of bombardments, without any shadow of *positive* proof that any such things as molecules exist.

As S. Caunizzana has said: "Some of the followers of the modern school push their faith to the borders of fanaticism; they often speak on molecular subjects with as much dogmatic assurance as though they had actually realized the ingenious fiction of Laplace, and had constructed a microscope by which they could detect the molecule and count the number of its constituent atoms."

Speaking of the "modern manufacturers of mathematical hypotheses," Mattieu Williams says: "It matters not to them how 'wild and visionary,' how utterly gratuitous, any assumption may be, it is not unscientific provided it can be vested in formulæ and worked out mathematically."

"These transcendental mathematicians are struggling to carry philosophy back to the era of Duns Scotus, when the greatest triumph of learning was to sophisticate so profoundly an obvious absurdity that no ordinary intellect could refute it. . . . The close study of *pure* mathematics, by directing the mind to processes of calculation rather than to phenomena, induces that sublime indifference to facts which has characterized the purely mathematical intellect of all ages."

Tyndall, however, states in all frankness, and without the aid of mathematical considerations, that "when we try to visualize the motions of the air having one thousand separate tones, to present to the eye of the mind the battling of the pulses, direct and reverberated, the imagination retires baffled at the attempt;" and he might have added, the shallowness and fallacy of the wave theory of sound was made apparent. He, however, does express himself as follows: "Assuredly, no question of science ever stood so much in need of revision as this of the transmission of sound through the atmosphere. Slowly but surely we mastered the question, and the further we advance, the more plainly it appeared that our reputed knowledge regarding it was erroneous from beginning to end."

Until physicists are willing to admit that the physical forces of nature are objective things—actual entities, and not mere modes of motion—a full and clear comprehension of the phenomena of nature will never be revealed to them. The motion of all bodies, whether small or great, is due to the entitative force stored up in them, and the energy they exercise is in proportion to the stored-up force.

Tyndall says that "*heat itself, its essence and quiddity, is MOTION, AND NOTHING ELSE.*" Surely, no scientist who considers what motion is can admit such a fallacious statement, for motion is simply "position in space changing;" it is a phenomenon, the result of the application of entitative force to a body. It is no more an entity than shadow, which is likewise a phenomenon. Motion, *per se*, is nothing and can do nothing in physics. Matter and force are the two great entities of the universe—both being objective things. Sound, heat, light, electricity, etc., are different forms of manifestation of an all-pervading force element—substantial, yet not material.

[NATURE.]

THE RELATION OF TABASHEER TO MINERAL SUBSTANCES.

MR. THISELTON DYER has rendered a great service, not only to botanists, but also to physicists and mineralogists, by recalling attention to the very interesting substance known as "tabasheer." As he truly states, very little fresh information has been published on the subject during recent years, a circumstance for which I can only account by the fact that botanists may justly feel some doubt as to whether it belongs to the vegetable kingdom, while mineralogists seem to have equal ground for hesitation in accepting it as a member of the mineral kingdom.

It is very interesting to hear that so able a physiologist as Prof. Cohn intends to investigate the conditions under which living plants separate this substance from their tissues. That unicellular algae, like the Diatomaceæ, living in a medium which may contain only one part in 10,000 by weight of dissolved silica, or even less than that amount, should be able to separate this substance to form their exquisitely ornamented frustules is one of the most striking facts in natural history, whether we regard it in its physiological or its chemical aspects.

Sir David Brewster long ago pointed out the remarkable physical characters presented by the curious product of the vegetable world known as "tabasheer," though so far as I can find out it has not in recent years received that attention from physicists which the experiments and observations of the great Scotch philosopher show it to be worthy of.

Tabasheer seems to stand in the same relation to the mineral kingdom as do ambers and pearls. It is in fact an *opal* formed under somewhat remarkable and anomalous conditions which we are able to study; and in this aspect I have for some time past been devoting a considerable amount of attention to the minute structure of the substance by making thin sections and examining them under the microscope. It may be as well, perhaps, to give a short sketch of the information upon the subject which I have up to the present time been able to obtain, and in this way to call attention to points upon which further research seems to be necessary.

From time immemorial tabasheer has enjoyed a very high reputation in Eastern countries as a drug. Its supposed medicinal virtues, like those of the fossil teeth of China and the belemnites ("thunderbolts") of this country, seem to have been suggested by the peculiarity of its mode of occurrence. A knowledge of the substance was introduced into Western Europe by the Arabian physicians, and the name by which the substance is generally known is said to be of Arabic origin. Much of the material which under the name of "tabasheer" finds its way to Syria and Turkey is said, however, to be fictitious or adulterated.

In 1788 Dr. Patrick Russell, F.R.S., then resident at Vizagapatam, wrote a letter to Sir Joseph Banks in which he gave an account of all the facts which he had been able to collect with respect to this curious substance and its mode of occurrence, and his interesting letter was published in the Philosophical Transactions for 1790 (vol. lxxx., p. 273).

Tabasheer is said to be sometimes found among the ashes of bamboos that have been set on fire (by mutual friction?). Ordinarily, however, it is sought for by splitting open those bamboo stems which give a rattling sound when shaken. Such rattling sounds do not, however, afford infallible criteria as to the presence or absence of tabasheer in a bamboo, for where the quantity is small it is often found to be closely adherent to the bottom and sides of the cavity. Tabasheer is by no means found in all stems or in all joints of the same stem of the bamboos. Whether certain species produce it in greater abundance than others, and what is the influence of soil, situation, and season upon the production of the substance, are questions which do not seem as yet to have been accurately investigated.

Dr. Russell found that the bamboos which produce tabasheer often contain a fluid, usually clear, transparent, and colorless or of greenish tint, but sometimes thicker and of a white color, and at other times darker and of the consistency of honey. Occasionally the thicker varieties were found passing into a solid state, and forming tabasheer.

Dr. Russell performed the interesting experiment of drawing off the liquid from the bamboo stem and allowing it to stand in stoppered bottles. A "whitish, cottony sediment" was formed at the bottom, with a thin film of the same kind at the top. When the whole was well shaken together and allowed to evaporate, it left a residue of a whitish brown color resembling the inferior kinds of tabasheer. By splitting up different joints of bamboo Dr. Russell was also able to satisfy himself of the gradual deposition within them of the solid tabasheer by the evaporation of the liquid solvent.

In 1791, Mr. James Louis Macie, F.R.S. (who afterward took the name of Smithson), gave an account of his examination of the properties of the specimens of tabasheer sent home by Dr. Russell (Phil. Trans., vol. lxxxi., 1791, p. 368). These specimens came from Vellore, Hyderabad, Masulipatam, and other localities in India. They were submitted to a number of tests which induced Mr. Macie to believe that they consisted principally of silica, but that before calcination some vegetable matter must have been present. A determination of the specific gravity of the substance by Mr. Macie gave 2.188 as the result. Another determination by Mr. Cavendish gave 2.169.

In this same paper it is stated that a bamboo grown in a hot-house at Islington gave a rattling noise, and on being split open by Sir Joseph Banks yielded, not an ordinary tabasheer, but a small pebble about the

size of half a pea, externally of a dark brown or black color, and within of a reddish brown tint. This stone is said to have been so hard as to cut glass, and to have been in parts of a crystalline structure. Its behavior with reagents was found to be different in many respects from that of the ordinary tabasheer; and it was proved to contain silica and iron. The specimen is referred to in a letter to Berthollet published in the *Annales de Chimie* for the same year (October, 1791). There may be some doubt as to whether this specimen was really of the nature of tabasheer. If such were the case, it would seem to have been a tabasheer in which a crystalline structure had begun to be set up.

In the year 1806, MM. Fourcroy and Vauquelin gave an account of a specimen of tabasheer brought from South America in 1804 by Humboldt and Bonpland (*Mem. de l'Inst.*, vol. vi., p. 382). It was procured from a species of bamboo growing on the west of Pichincha, and is described as being of a milk white color, in part apparently crystalline in structure, and in part semi-transparent and gelatinous. It was seen to contain traces of the vegetable structure of the plant from which it had been extracted. On ignition it became black, and emitted pungent fumes.

An analysis of this tabasheer from the Andes showed that it contained 70 per cent. of silica and 30 per cent. of potash, lime, and water, with some organic matter. It would, perhaps, be rash to conclude from this single observation that the American bamboo produced tabasheer of different composition from that of the Old World; but the subject is evidently one worthy of careful investigation.

It was in the year 1819 that Sir David Brewster published the first account of his long and important series of observations upon the physical peculiarities of tabasheer (Phil. Trans., vol. cix., 1819, p. 283). The specimens which he first examined were obtained from India by Dr. Kennedy, by whom they were given to Brewster.

Brewster found the specimens which he examined to be perfectly *isotropic*, exercising no influence in depolarizing light. When heated, however, it proved to be remarkably *phosphorescent*. The translucent varieties were found to transmit a yellowish and to reflect a bluish white light—or, in other words, to exhibit the phenomenon of *opalescence*. When tabasheer is slightly wetted, it becomes white and opaque; but when thoroughly saturated with water, perfectly transparent.

By preparing prisms of different varieties of tabasheer, Brewster proceeded to determine its refractive index, arriving at the remarkable result that tabasheer "has a lower index of refraction than any other known solid or liquid, and that it actually holds an intermediate place between water and gaseous bodies!" This excessively low refractive power Brewster believes to afford a complete explanation of the extraordinary behavior exhibited by tabasheer when wholly or partially saturated with fluids. A number of interesting experiments were performed by saturating the tabasheer with oils of different refractive powers, and by heating it in various ways and under different conditions, and also by introducing carbonaceous matter into the minute pores of the substance by setting fire to paper in which fragments were wrapped.

The mean of experiments undertaken by Mr. James Jardine, on behalf of Brewster, for determining the specific gravity of tabasheer, gave as a result 2.235. From these experiments Brewster concluded that the space occupied by the pores of the tabasheer is about two and a half times as great as that of the colloid silica itself!

From this time forward Brewster seems to have manifested the keenest interest in all questions connected with the origin and history of a substance possessing such singular physical properties. By the aid of Mr. Swinton, secretary to the government at Calcutta, he formed a large and interesting collection of all the different varieties of tabasheer from various parts of India. He also obtained specimens of the bamboo with the tabasheer *in situ*. In 1828 he published an interesting paper on "The Natural History and Properties of Tabasheer" (*Edinburgh Journal of Science*, vol. viii., 1828, p. 288), in which he discussed many of the important problems connected with the origin of the substance. From his inquiries and observations, Brewster was led to conclude that tabasheer was only produced in those joints of bamboos which are in an injured, unhealthy, or malformed condition, and that the siliceous fluid only finds its way into the hollow spaces between the joints of the stem when the membrane lining the cavities is destroyed or rent by disease.

Prof. Edward Turner, of the University of London, undertook an analysis of tabasheer, the specimens being supplied from Brewster's collection (*Edinburgh Journal of Science*, vol. viii., 1828, p. 335). His determinations of the specific gravities of different varieties were as follows:

Chalky tabasheer	2.189
Translucent tabasheer	2.167
Transparent tabasheer	2.160

All the varieties lose air and hygroscopic water at 100° C., and a larger quantity of water and organic matter (indicated by faint smoke and an empyreumatic odor) at a red heat. The results obtained were as follows:

	Loss at 100° C.	Loss at red heat.
Chalky tabasheer.	0.838 per cent.	1.277 per cent.
Translucent tabasheer 1.620 " "	" "	3.840 " "
Transparent tabasheer 2.411 " "	" "	4.518 " "

Dr. Turner found the ignited Indian tabasheer to consist almost entirely of pure silica with a minute quantity of lime and vegetable matter. He failed to find any trace of alkalis in it.

In 1855, Guibourt (*Journ. de Pharm.* [3], xxvii., 81, 161, 252; *Phil. Mag.* [4], x., 229) analyzed a specimen of tabasheer having a specific gravity of 2.148. It gave the following result:

Silica	= 96.94
Potash and lime	= 0.13
Water	= 2.93
Organic matter	= trace

Guibourt criticised some of the conclusions arrived at by Brewster, and sought to explain the source of the silica by studying the composition of different parts of the bamboo. While the ashes of the wood contained