agree with the results obtained by prismatic decomposition; while the results in the other case do not. We think it would be correct to say that iridescence does not reveal non-luminous bodies in the same way, nor with the same certitude, as that light reveals them by which they are ordinarily visible. In making this last statement we have in mind the fact that the iridescent surface, in addition to its iridescence, also emits or radiates light in the same manner as ordinary visible bodies; and that these two facts are not to be confounded in our observations and reasonings. Without pursuing the subject further into details, these are some of the reasons why we think the facts of iridescence are not inconsistent with the main doctrine of this paper.

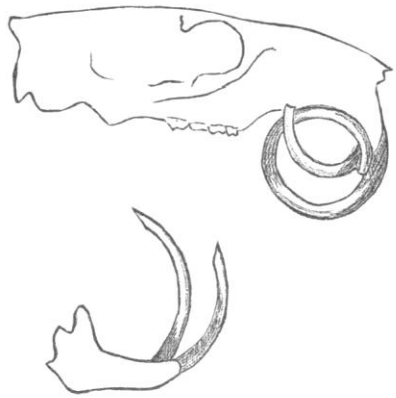
We conclude then, by reason of the facts and relations to which we have now called attention, we cannot believe that it is correct to say that non-luminous bodies are seen by *reflected light*; and we offer the suggestion that the light by which such bodies are seen should fairly and properly be called *radiant light*, as manifesting all the essential qualities of such light.

CORRESPONDENCE.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

To the Editor of "SCIENCE."

In an article on overgrown teeth of Fiber Zibethicus (which by a singular typographical error is printed Fiber Wibethicus) in "SCIENCE" for July 16th, the writer describes a not very uncommon phenomenon among rodents to which I can add an interesting example.



The inclosed drawing represents a similar case, being a woodchuck (Arctomys monax); it will be noticed that one of the upper teeth has grown far enough to form a semicircle while the other upper incisor has described a somewhat larger curve and finally thrust itself through the first and then continued to form a *complete circle*, as will be evident from the figure. This specimen was mounted here (with one other similar but not so extreme a case) and is now in the Museum of Comparative Zoology at Cambridge. F. W. STAEBNER.

July 20, 1881.

WARD'S NATURAL SCIENCE ESTABLISHMENT, Rochester, N. Y.

COMET (c) 1881.

To the Editor of "SCIENCE."

The comet discovered by Mr. Schaeberle at Ann Arbor, July 13, promises to become a very interesting object, not only because it will soon be visible to the naked eye, but also because its orbit shows great similarity to the great comet of 1337, as may be seen by the following comparison:

	1881 (Stone)	1337 (Hind)	1337 (Lanzier)
Distance of perihelion from node	122° 30′	108° 44′	90° 41′
Longitude of node	98 43	99 6	93 1
Inclination	141 35	137 6	139 32
Logarithm perihelion distance	9.7959	9.97	9.92

The difference between the orbits of the two comets is perhaps not greater than the uncertainty of that of 1337. The latter was first seen in China on the 26th of June, and afterwards in Europe on the 24th of October.

Schaeberle's comet has been observed here on a number of mornings, and its increase in brightness has been quite perceptible. This morning the tail was very apparent, the sky was very cloudy, or I presume it would have been visible to the naked eye. It ought to be quite plainly visible at any rate before the end of this week. It will be at perihelion and nearest the earth about the 20th of August, and will remain at approximately the same distance from us for a week or more. A few days before that time its right ascension will have become equal to the sun, so that when at its greatest brilliancy it will be visible in the evening. While it will undoubtedly become a magnificent object, it will not probably equal the great comet now receding from us.

ORMOND STONE.

MT. LOOKOUT, O., *July* 25, 1881.

ASTRONOMICAL NOTE.

WASHBURN OBSERVATORY, UNIVERSITY OF WISCONSIN, MADISON, WIS., July 17, 1881.

To the Editor of "SCIENCE."

Among the new red stars found here, the following is by far the finest and may be of interest:

Anon. 9 mag. R. A. $1^h\ 48^m\ 45^s$; Dec. = $+\ 58°\ 40'.2$ 1880.0. EDWARD S. HOLDEN.

ADULTERATION OF SUGAR.

To the Editor of "SCIENCE."

DEAR SIR—In the leading editorial of "SCIENCE" of June 18, you speak of the different results obtained by Prof. Leeds and myself of examination of commercial sugars and syrups for glucose and grape sugar. I can only take exception to one statement contained therein, *i. e.*, the one which intimates that these different results form the theme of a scientific controversy. Since the reception of your letter I have renewed my inquiries for statistics, and can now say that I do not believe my estimates of the quantities made in the United States are very wide of the truth. Dealers and manufacturers are extremely reticent on the whole subject, and it is only by hard work and often indirection, that one can get at the truth. In your own city, New York, there is a large establishment for making "New Process Sugar," the Manhattan Refining Company, unless it has lately changed its name. Yet a prominent New York chemist stated publicly, and published over his own signature, that he had made diligent search for this establishment, and it could not be found. At the same time, to my personal knowledge, a western firm had just received a large consignment of "New Process Sugar" from this firm.

At the Boston meeting of the A. A. A. S., I stated on the strength of this personal knowledge that I believed the Manhattan Company was no myth. This statement was published in the Boston and New York papers, and was seen by the proprietors of the Manhattan Company. They wrote to assure me that I was right in my statement, sending me at the same time samples of their different sugar for analysis.

Within the past year the mixing of sugars has largely increased, and is now carried on in New York, in Buffalo, in Chicago, and at other points. A prominent sugar dealer has just told me "some of these establishments turn out five hundred barrels daily."

From the best information I can get, I would place the daily yield of mixed sugars at 1500 to 2000 barrels. This, remember, is only a careful estimate from all the data I can get. The process is increasing just as fast as *dry white* grape sugar can be made.

With regard to table syrup, I can only reiterate my former statements. In response to a recent inquiry a prominent dealer has just written me as follows:

"My observation leads me to believe that fully four-fifths of all the syrup sold throughout the western and northwestern States and Territories are composed of glucose, with enough cane syrup or molasses added to give it the color most desirable to the buyer or consumer."

I have no accurate information respecting the eastern trade. I am not without a suspicion, however, from the appearance of some syrups which I have lately seen on Boston tables, that the beautiful syrup made from the corn of our western prairies, has invaded the very stronghold of the dirty refuse syrups of the sugar refineries.

Respectfully,

H. W. WILEY.

BOSTON, *July* 27, 1881.

WATER AS FUEL.

To the Editor of "SCIENCE":

I am interested in the paper of Dr. Rachel, in "SCIENCE," July 9, on the use of water in combustion.

The subject is a most important one; and I regret a mistake which a reader might fall into from an inadvertency, I suppose, in not more clearly distinguishing between the degrees of temperature at which the transfer of oxygen takes place from the hydrogen of the water to the carbon set free by the dissociation of the naphtha, and the number of heat units set free or absorbed by such transfer, which is a very different thing.

It is not a case of the dissociation of water, but of the dissociation of naphtha, and the *transfer* of oxygen from the hydrogen to the carbon so set free; a carbon which was very loosely held by the hydrogen in naphtha.

It is only a trade of so much carbon for so much hydrogen; the absolute heat of which, according to authorities, are almost exactly equal in complete burning.

But while the trade is thus equal in absolute heat, there is practically an enormous gain; and it is very important to see just what it is.

We get our heat all in hydrogen instead of in carbon, and so avoid the enormous practical losses which attend the usual mode of burning carbon.

We get our heat in hydrogen, which is the easiest of all things to get all of it out by burning, instead of in carbon which is one of the hardest of all things to get it all out of, the difference being much like that between the ease of getting our money out of a bank or out of a lot of debts where from half to three-fourths or nine-tenths is almost always finally lost.

The practical man does not understand very well what it means when he is told that the use of coal under boilers only produces five per cent of its energy in work. It means this: that a series of enormous losses is made in trying to get the heat out of the carbon in burning. An enormous quantity goes off as black smoke and soot, not burned. An enormous quantity goes off as carbonic oxide, giving up only a third of its heat, instead of giving it all up by burning to carbonic acid. An enormous quantity of heat is lost in heating up the great quantity of gases and air, that pass off without being fully burned, which prevents a high temperature. An enormous quantity of heat is carried up the chimney; because a little coat of soot and ashes on the boiler does not allow it to go into the boiler fast enough while passing so rapidly to the chimney. Is it any wonder that an enormous revolution is to be made by a mode of burning where water is used not for any energy to be got out of it, but by which the heat is taken from carbon, and put into hydrogen, where it all flashes into usable form the instant the air can reach it? Where the whole heat is liberated at one point where it is wanted, instead of in a long, imperfect flame? Where there is not a particle of soot or dust to tarnish the boiler and prevent the heat from passing into it wherever it strikes? And where the heat is nearly all in that low form of invisible radiance which is best suited to radiate on to, and be absorbed by, the boiler without waiting for the heated gases to actually strike it to give up their heat by slow convection?

It is a fact well known in the use of the alcohol and gas flames, that if you want them for heating alone you want the non-luminous flame only; because with a luminous flame a large part of the heat is in the form of light, which is not so readily absorbed by substances as heat, as it is with the non-luminous flame; and another large part is held by the gases as convective heat, and cannot so freely pass into substances by radiation, as in the radiation from the non-luminous flame.

Thus the revolution in combustion by the use of water consists in transferring the heat from carbon to hydrogen, by which all the great losses of burning are avoided, and the entire heat is obtained clean, without smoke, at the exact point desired, and in the form which takes right hold of the work without loss of time or energy.

These things are of an incalculable value; very much better than any mysterious supposed increase of heat from the water itself.

Though it is important to form a non-luminous gas by means of water to utilize the carbon, yet the water so added takes up a share of the heat to raise its temperature along with the other gases, and so reduces the temperature in proportion to its quantity.

So, if we need high heats we must use the least quantity of water, which will turn all the carbon into a suitable gas.

If we add water enough to make the carbon of 100 pounds of naphtha into carbonic acid, it will take 250 pounds of water, if we assume that the naphtha averages a composition of $C_6 H_{14}$, containing 84 pounds of carbon and 16 pounds of hydrogen; and the result would be 30 pounds of hydrogen and 310 pounds of carbonic acid. This would require 1530 pounds of air to burn it, and produce 1880 pounds of gas, of which over one-seventh would be due to the water, and the temperature would be less than $\frac{5}{7}$ of what it would have been without, if an equally perfect burning could have been secured without; because the water, as steam, has about twice the capacity for heat, as the other gases.

If we only add enough water to make all the carbon into carbonic oxide, it will take only half as much water; 125 pounds to 100 pounds of naphtha making about one-thirteenth of the gas after burning; and reducing the temperature only one-seventh.

But if we use only 36 pounds of water to 100 pounds of naphtha, one gallon of water to four gallons of naphtha, the gases may be something like this:

Marsh gas, CH_4, 80 pounds.
Carbonic oxide, CO, 56 pounds.

which with 1530 pounds of air (20,000 cubic feet) to burn it, will make 1666 pounds of gas; and the water added will only reduce the temperature one-twenty-fifth part, instead of so much as before.

In each case the air to burn it, and the units of heat produced, will be the same; but the temperature will vary with the proportion of water added, and, also, with the quantity of unburned air passing through.

One of the great advantages of the water process is the condition of blast with which the fuel unites with the air, by which the thorough mixture and burning is secured. And it is very important that the quantity of air going into the furnace be regulated so as to furnish only about the right amount. All that is more or less than the exact amount required, reduces the temperature so much.

In ordinary furnaces, it has been estimated that much more than half the heat is lost by this one item alone. If the air passes freely in above the coal, twice as much goes in as is burned; if it all passes in under the grate, then only one-third the heat is given off, as only carbonic oxide escapes.

Probably the advantages with crude petroleum or with coal, of the water process, would be of still greater value.*

SAMUEL J. WALLACE.

WASHINGTON, D. C.

BOOKS RECEIVED.

OBSERVATIONS OF DOUBLE STARS made at the United States Naval Observatory by ASAPH HALL, Professor of Mathematics, U. S. N. Rear Admiral Rogers, U. S. N., Superintendent, Washington, 1881.

In introducing this work Professor Hall gives some very interesting details respecting the methods used in making observations at the Naval Observatory and the condition of the instruments.

He also presents his reasons for undertaking these observations and indicates the scope of the present work.

He states that his regular observations with the 26-inch refractor of the Naval Observatory were begun in the spring of 1875, the instrument at that time being in charge of Professor Simon Newcomb. "Professor Newcomb gradually withdrew from observing with this instrument, which came under my direction sometime in July of the same year. The micrometrical measurements which had been made by Professors Newcomb and Holden were chiefly of the satellites of Uranus and Neptune, and the discussion of these measurements of the two outer satellites of Uranus brought out very clearly what had been indicated before by Von Asten; viz, the existence of a large constant difference in the angles of position measured by Mr. Otto Struve, director of the Imperial Observatory at Pulkowa. As it is our intention to repeat the measurements of the satellites of Uranus and Neptune after a few years, and as it seemed probable that similar differences might exist in the observations of double stars, it occurred to me that the best way of comparing and uniting the observations of different astronomers would be for each one to observe the same double stars at nearly the same time. I wrote to Struve proposing that this should be done, and that he should select the list of stars. In reply he informed me that such a series of observations was already in progress between himself and Baron Dembowski, and after adding to the list of stars a few of greater distances, this list and an account of the proposed work were published by Struve in the "Vierteljahrsschrift der Astronomischen Gesellschaft." Band xi, p. 227.†

It was understood that each observer should avoid all knowledge of the observations of other astronomers, in order that his work might be done independently, and in my own case this rule has been carefully adhered to. But now nearly four years have elapsed since Struve's publication, and it is probable that all the astronomers engaged in this work have collected such a number of observations that the publication of my own results will not influence the independence of theirs. Moreover, the end of the year 1879 seems to be a favorable epoch for publishing my observations of double stars made before 1880, since I hope to make some changes which in the future will enable me to observe under conditions more favorable to accuracy.

I have therefore collected and revised all my observations of double stars, and the results are given in the following pages. In order to make this collection complete I have concluded the few observations made in the year 1863 with the equatorial of 9.6 inches aperture. The whole number of observations is 1614.

It will not be necessary to give any general description of the 26-inch refractor made by Alvan Clark and Sons for the Naval Observatory, since such descriptions can be found in the annual volumes of the Observatory for 1873 and 1874. It will be sufficient to say that the form of the mounting adopted by the makers for this Equatorial is such that the instrument, notwithstanding its great size, is handled with ease; and the harp-shaped piece that supports the polar axis is very convenient when observing near the zenith. Generally the instrument is pointed on a star by means of what are called the "rough circles." These circles are the edges of the hour and declination circles, which were painted white, and then divided by lines of black paint, the hour circle into spaces of ten minutes of time and the declination circle into degrees. This method of pointing is usually accurate enough to find the object, but as the painting was not well done errors as great as 15′ to 20′ could be made in some parts of the rough declination circle. An accurate reading for the position could be made by means of the finely divided circles, but this involves considerable time and trouble. On account of the delay in the observations which would be caused in making the change, and of the natural inertia in getting rid of a poor thing to which one has become accustomed, this defective circle for the declination was used until June, 1879, when the circle was painted white and divided again under the care of Mr. Gardner, the instrument maker of the Observatory. The settings are now much more accurate and give but little trouble, and the saving of time is very great. It is possible that a few cases may be found where, on account of an erroneous setting in declination, I have observed a different object from the one supposed.

The ease and rapidity with which observations can be made with a filar micrometer depend largely on the performance of the driving-clock. The accuracy of the observations also is in a measure dependent on this performance, but patience and skill on the part of the observer will in a good degree make up for a poor performance of the clock. The motive power of our driving-clock comes from a small water-wheel which is driven by water drawn from the Potomac water pipes. At first the water was applied directly to the conical pendulum, but the pressure of the water was so variable that weights attached to an endless cord (Huygen's loop), were placed between the water-wheel and the pendulum by Professor Newcomb. When this had been done the performance of the clock is said to have been tolerable; but in the autumn of 1875 it became very troublesome, and the observer was frequently annoyed by the stopping of the clock. This trouble continued and became worse until July, 1876, when the clock was dismounted by Mr. Gardner and myself. The lower end of the shaft of the conical pendulum had been given a conical shape, and had rested in a conical cup. The friction and heat had been so great that the lower end of this shaft had become very rough and twisted to a gimlet shape, thus stopping the clock. The bearing of the shaft was changed and made of a plane agate surface, the lower end of the shaft being rounded to a slightly curved surface. The friction of the upright shaft of the water-wheel was also diminished by clamping a set of friction wheels to this shaft and letting them play on a horizontal iron surface. The weights on the Huygen's loop were changed for cups carrying shot. With an average pressure of the water, and the machinery well oiled, these

* This superiority of the non-luminous combustion for heating was discovered by Professor Henry. He says: "With this arrangement the light of the flame was increased, while the time of bringing the water to the boiling point was also commensurably increased, thus conclusively showing that the increase of light was at the expense of the diminution of the temperature."

† Mittheilung über unternommene Beobachtungsreihen zur Vergleichung von Mikrometer messungen. 1876, Anfang Juni. OTTO STRUVE.