molecular motion in the surrounding medium. And the brilliant incandescence in Geissler, Crookes, and Tyndall tubes from minute increments of energy are well known.

This increase of temperature and radiation from small increments of energy in highly tenuous matter seems to be what we ought to expect from the phenomena of this force or energy when it is in the form of molar motion. We then measure it by the mass and velocity of the moving body; that is, by its momentum, and this momentum is what is convertible into heat when the movement is resisted.

Increase in velocity compensates for decrease in mass, and hence a small projectile, at high velocity, will do the same work as a larger projectile at lower velocity; and the momentum, in each case, can be converted into the same units of heat. For obviously the same reason, the intense velocity imparted to the gaseous products of an explosion of dynamite enables this highly tenuous matter to do precisely the same work on a hard rock, as a hammer of a million times the mass, but moving with only one-millionth of the velocity.

But there is necessarily a limit to this substitution of velocity for mass; and this limit is in the capacity of matter to embody the energy; and when the force of energy is applied to matter in the form of heat we ought to expect to find the same limit. This application in the form of heat may be made by conduction, when the whole energy imparted is absorbed; or by radiation when only so much as is not reflected, is absorbed; but the resulting phenomena are the same, whatever may be the process by which the absorption is accomplished.

The fact developed in spectrum analysis, that incandescent matter absorbs the same rays of light which it emits, seems to be another illustration of the law that the capacity of matter to receive radiant energy is limited, and in this case by its capacity to radiate the energy received.

If the evolution of heat and elevation of temperature results from resisted molecular motion, it necessarily follows, that a single molecule, moving in unconfined space, whatever may be its velocity, would be at the absolute zero of temperature. But this is mere speculation of no scientific value, because we have no evidence that a molecule can become separated from other molecules, nor that it is possible to place it where it could move without resistance.

But there is another induction of practical importance in sustaining the assumption that we have just made. If the effect of heat imparted to matter by conduction or radiation is to set up the molecular motion evidenced by expansion, and this work of molecular motion must be resisted before radiation begins, it necessarily follows that the number of molecules in the body receiving heat, and to which motion can be imparted; in other words, the density of tenuity of the matter, must be an element, determining, in some measure, the capacity of the matter to absorb heat.

This explains why the atmosphere decreases in temperature with increase of tenuity, upwards from the earth's surface; and why we can assume absolute zero in space entirely unoccupied by ponderable matter, if there is any space thus entirely unoccupied, notwithstanding the presence of potential or dynamic energy, because it is only in conjunction with ponderable matter (resisted molar or molecular motion) that dynamic energy develops elevation of temperature, and the other phenomena of heat.

It is obvious that force or energy in the form of molar motion is being constantly converted by impact or friction into the form of heat. Taking the earth as a whole, during the period of human observation, this constant conversion of molar motion into heat has been compensated by a conversion of heat into molar motion, so that the equilibrium between the two forms of this force or energy has been preserved in terrestrial nature, and there has been no loss of motion nor increase of heat, since man began to observe nature and keep a record of his observations.

Resistance to movement, that is, to the work being done by the force or energy in molar motion, is necessary to convert the force or energy into the form of heat; and it may be that when this force of energy is applied to ponderable matter in the form of heat, and its proper work as heat is resisted, the surplus heat may be converted directly into molar motion.

It is certainly within the range of possibility, that, under certain conditions, a body of ponderable matter may receive increments of heat more rapidly than it can furnish work for it in the molecular motion of expansion, or discharge it by radiation or conduction; and, in such case, it seems inevitable that the body thus receiving more heat than it could furnish work for or discharge, if free to move, would be put in motion away from the source of heat, and that this motion would continue until a distance from the source of heat was reached, at which the heat received was not greater than could be employed in expansion or discharged in radiation and conduction.

Dr. Grove was inclined to the opinion that it was thus possible to convert heat directly into molar motion. He says, "There are, indeed, some delicate experiments which tend to prove that a repulsive action between separate masses is produced by heat. Fresnel found that mobile bodies heated in an exhausted receiver repelled each other to sensible distances; and Baden Powell found that the colored rings, usually called Newton's rings, change their breadth and position, when the glasses between which they appear are heated, in a manner which showed that the glasses repelled each other." [1]

But, however that may be, there is certainly a molar motion which always follows and evidences the molecular motion of expansion. The law that action and reaction must be equal and opposite, applies to molecular motion in a closed vessel. It is the operation of this law which secures uniform pressure in steam boilers, and other like devices for using gas expansion for mechanical purposes; and thus converts the molecular motion, evidenced by expansion, into molar motion.　　　　DANIEL S. TROY.

(To be continued.)

LETTERS TO THE EDITOR.

A Question in Physics.

CAN there be a crowding of the particles of a gas to a much smaller compass without its being markedly heated? Can a gas expand without being cooled? At first thought the answer would seem to be an emphatic no in both cases; but it would appear that these conditions may exist sometimes. Science, Vol. XV., p. 387, published the results obtained by direct determination of the heating of air when compressed by a pump connected by a long tube with the cylinder. A compression to ten inches above atmospheric pressure gave a heating of about 4° F., ignoring the heat lost to the sides of the cylinder. The corresponding expansion into the open air gave a cooling of about 4°, neglecting the

[1] "Correlation and Conservation of Forces," p. 41. D. Appleton & Co. 1890.

heating effect of the cylinder. These results were strongly com-
bated by Professor Ferrel in *Science*, Vol. XVI., pp. 192 and 193,
and also ˌby Professor Marvin. Professor Ferrel published the
well-known thermo-dynamic formula, given in *Science* for Feb.
19, and applying it to the heating in the above case found it 43°
F. instead of the 4° found by the experiment. It would seem,
however, that these experiments had not been controverted, and
it is probable that their justness may yet be established. This
problem is far-reaching in its application, and it is for this reason
that it is dwelt upon at some length.

The formula given by Professor Ferrel applies only in cases
where a gas is compressed directly by an external force, and when
all the heat developed in the work of compression is concentrated
in the gas. One of Joule's experiments will serve to elucidate this
point. He determined the mechanical equivalent of heat by im-
mersing the cylinder into which the air was to be compressed and
the compressing pump in the same water bath, and then deter-
mining the amount of compression and the total heat developed.
This shows at once the truth of the following proposition. If a
gas 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 enter it. This proposition seems self-evi-
dent; nevertheless, it would seem that nearly all the errors that
have entered the various discussions and theories regarding this
matter have arisen from a neglect of this obvious statement.

We may analyze Joule's experiment in order to gain a clearer
understanding of the problem. Suppose the compressing pump
had been in a bath by itself, and the cylinder in another bath;
also that no heat was lost in the passage of the air from the pump
to the cylinder. Under these circumstances a good deal of the
heat due to the action of the pump would have passed into its
bath, and only a small portion would have been carried by the hot
air into the cylinder. Let us consider that a certain definite
amount of heating would have taken place if all the heat had en-
tered the air in Joule's original experiment, the formula gives the
rise as 123° F. if the initial temperature of the air had been 60°,
and the compression was to two atmospheres. In the present in-
stance, however, most of the heat would have been absorbed by
the bath around the pump, and would not have been available for
heating the compressed air in the cylinder. It is impossible to
consider that the same amount of work would have sufficed to
heat the water around the pump, and then would have developed
heat enough to raise the temperature of the air in the cylinder
123°.

Again, suppose that the compressed air, before entering the cyl-
inder, had its temperature lowered to the outside temperature; is
it not plain that all the heat developed in the work of compression
would be disposed of, and none at all would be available for heat-
ing the compressed air? We see, then, that it is entirely feasible
to bring about certain conditions under which a gas may be greatly
compressed without being heated.

Let us take two equal cylinders connected by a tube and com-
press the air in one, A, to three atmospheres, the air in the other,
B, being at atmospheric pressure. Let the air in A be at the tem-
perature of the outside air. On opening communication between
the cylinders the air in A will be slightly chilled, owing to the
work of imparting a certain velocity to those particles rushing
into B; while the air in B will be heated slightly from the impact
of the particles rushing out of A. All the heat due to the work
of compression, however, will have disappeared, and none will be
available for heating the air in B (See Enc. Brit., Vol. XXII., p.
480, section 34).

Lastly, suppose that the air in A should be allowed to escape
into the open air; the resistance to the rush of the air would be
much less than in the last case, and hence a greater velocity
would be imparted to the particles rushing from A, and the cooling
would be slightly greater than before. The situation appears very
plain, and there is no difficulty now in understanding why the ear-
lier experimental heating and cooling was only 4°.

These views seem almost startling in their nature, and if true
certainly have profound significance. Let us try to picture the
real condition of the gas when under compression and flowing
from one reservoir to another. The confined air has a certain po-

tential energy and a capacity for work; it may flow into any res-
ervoir where the air is at atmospheric pressure without losing its
potential energy, and hence, if none of its energy is lost, it cannot
be used up in heating the air. Is it not like the water in a pond
having a certain head or capacity for work? We may enlarge
the pond, and allow the water to flow over a larger area; but the
capacity for work will be diminished very slightly.   X.

Feb. 23.

The Balloon Problem.

THE problem of the amount of work done by the gas in a bal-
loon expanding as the balloon rises, as proposed in *Science* for
Feb. 19, may be much more significant than even the proposer
has thought. Take a bag perfectly flexible and holding two cubic
feet. Force out all the air and tie the neck. If we attempt to
separate the sides, we shall find it impossible to do so; as
the air presses upon it fifteen pounds to the square inch. Allow
a cubic foot of dry air to enter and again close the bag. We
shall find the same difficulty as before in further opening the bag.
Consider that the air in the bag has been heated 490°, which will
just fill the bag. To separate the molecules has required a work
equivalent to lifting 2,160 pounds one foot, and for convenience
we say that the gas in expanding has lifted the weight of the at-
mosphere. Is it proper, however, to think of the outside air as
having been lifted? Has any more outside air been lifted than
the 1.2 ounces that a cubic foot weighs? The work, then, has
been internal and not external. This is a very important distinc-
tion. The external work has been only that required to lift the
weight of air displaced.

This can be shown best, perhaps, by determining just how much
change has taken place in the behavior of the bag to outside in-
fluences. If any external work has been done, we ought to be
able to measure it. If the bag with its two cubic feet of air were
ᵃleft to itself, it would soar aloft, and it would require a weight of
just 1.2 ounces to restrain it. We say the heated air displaces
two cubic feet of air at the outside temperature; and since its
density is just half that of the outside air, it can lift a weight
equal to that of one cubic foot of air.

Instead of heating the air, let us connect the empty bag with a
reservoir having a gas which has a density just half that of the
air. Here the conditions are entirely changed. The reservoir, to
all intents and purposes, is connected with the outside air, and
when we connect the mouth of the bag with it, there is no more
work required to expand the bag than if we had opened it into the
outside air. In the case before, after closing the bag, we could
not open it till some internal work had been done in expanding
the air; but now that internal work is not needed, and the only
work done by the gas in expanding the bag is that required to lift
one cubic foot of air one foot. The lifting power of the bag is
precisely the same as it was when it contained air at 490°. The
amount of external work in expanding the bag, or capacity to do
external work, is exactly the same.

Take the same bag, empty as at first, and connect it with a
reservoir containing two cubic feet of air at the outside tempera-
ture but at a pressure of two atmospheres. The air will flow
quickly into the bag and an equilibrium will be established with
the pressure at one atmosphere in both the reservoir and bag.
How much external work has been done? Has the air in expand-
ing lifted an enormous weight? Certainly not; the external work
has been equal to that required to lift two cubic feet of air, or 2.4
ounces, one foot. Here again we have entirely different condi-
tions from those in the first case. On connecting the bag with
the reservoir we virtually opened it to the outside air, and the out-
side air did all the work which in the first case was needed to be
done in separating the particles of air, or in increasing their
kinetic energy. We can see this at once by the following consid-
erations. Open the bag into the free air; we can pull the sides
apart to their fullest extent. Now connect the opened bag with
the reservoir which has the air at the outside pressure, the condi-
tions remain exactly as before, when the mouth of the bag was
open to the outside air. Empty the bag and connect it with the
reservoir. No change will take place, but the reservoir will vir-
tually be connected with the outside air. Now gently force air