heating effect of the cylinder. These results were strongly combated by Professor Ferrel in *Science*, Vol. XVI., pp. 192 and 193, and also by 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 immersing the cylinder into which the air was to be compressed and the compressing pump in the same water bath, and then determining 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-evident; 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 entered 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 instance, 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 cylinder, 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 heating 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 compress 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 temperature 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 earlier 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 potential energy and a capacity for work; it may flow into any reservoir 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 balloon 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 atmosphere. 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 distinction. 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 influences. 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 left 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 temperature 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 expanding 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 conditions from those in the first case. On connecting the bag with the reservoir we virtually opened it to the outside air, and the outside 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 considerations. 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 conditions 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 virtually be connected with the outside air. Now gently force air into the reservoir; the connection of the bag with the outside air will remain as before, and when the bag is full the only work external to the reservoir will be that of lifting 2.4 ounces one foot.

When a balloon rises into the atmosphere, then, the gas does not expand, and in so doing perform an enormous amount of external work; but it simply displaces the air. The amount of work in this case would be very small indeed, and the consequent cooling of the gas slight. The conditions are precisely similar to what they were when we connected the bag with our reservoir having the air under pressure. In rising, the balloon continually arrives at a region in which the pressure is less and the expanding gas simply displaces the surrounding air. Every cubic foot expansion in the gas of the balloon at sea level displaces a cubic foot of air at a pressure of thirty inches. If the pressure of the outside air were suddenly diminished to ten inches, the work done would be that of lifting a gas weighing one-third of the air at normal pressure, or about .4 ounces to each cubic foot. This would cause almost an inappreciable cooling in the gas.

A very interesting point may be mentioned in this connection. What became of the energy stored in the reservoir in the air compressed to two atmospheres, after the air had expanded to normal pressure in the reservoir and bag? PARADOX.

February 26, 1892.

The Loup Rivers in Nebraska.

I AM gratified that my article of Jan. 29 possessed some interest for so able an authority as Professor W. M. Davis of Harvard, albeit, he is somewhat critical.

My main propositions, and I think they will stand, notwithstanding the objections of my critic, are these: —

1. The Loup rivers were probably once "separate tributaries of the Platte, all independent of each other, as roughly indicated by the dotted lines on the map" (Fig. 1, p. 59, *Science*, Jan. 29, 1892).

2. Pliocene lacustrine deposition along the Platte "crowded the mouths of these tributaries eastward and made them coalesce into a single large tributary."

3. Headwater erosion "swept the upper courses westward by a series of captures."

Instead of my first proposition, Professor Davis ascribes to me the postulate "that at the beginning of the current cycle of river history the several branches of the Loup River all pursued independent courses to the Platte." He makes definite my indefinite "once," but not in a way that I can accept. The plain inference from my second proposition is that the period of separate existence of these tributaries was in the Miocene.

Whether that is equivalent to the "postulate" of Professor Davis depends upon the definition of "cycle." The facts, as I have read them in the field, are these: In Miocene times tributaries of the Platte, now constituting the Loup system, were developed only to the stage of young rivers, not mature rivers, as Professor Davis supposes. Then came submergence and partial obstruction of their valleys; partial only, because the Pliocene marls will not average more than fifty feet in thickness, not one-fourth of the depth of the valleys. When Lake Cheyenne retired, the rivers resumed business in their former channels, except near the Platte, where the excessive deposition turned them eastward. The silt in the Platte valley has been penetrated to the depth of five hundred feet without reaching the bottom.

Here then is a cycle of river history interrupted in its infancy, and subsequently resumed. Its course was not half run when the rivers were drowned, and, even now, after their emergence and resurrection, they are still young rivers, with abundant vigor and abundant opportunities for headwater erosion and river piracy. If this series of events may be accounted a single cycle, notwithstanding the Lake Cheyenne episode, then I can adopt the "postulate" as equivalent to my first proposition.

If I understand him aright, Professor Davis does not raise any objections to my second proposition. He does indeed argue against a supposed contention of mine, which is not mine at all, namely, that the coalescence of the lower courses into one Loup River was due to headwater erosion.

The effects which I did assign to headwater erosion were limited to the "upper courses," as stated in the third proposition. In spite of all objections, that proposition seems to be reasonable and valid. No region on this continent is more favorable for the study of simple, unobstructed headwater erosion than these western plains. The rivers are young. Great blocks of table lands lie yet unbroken by drainage lines, and into these fresh ravines are constantly eating back. The tertiary beds are soft and practically homogeneous, so far as resistance to erosion is concerned, so that no question need be raised about dip, strike, folds, or alternations of hard and soft strata. Upon such a terrane the Miocene rivers established themselves with a south east course consequent upon the slope to the south-east. The Rocky Mountain upheaval, together with excessive deposition along the Platte, changed the slope to the north-east, transverse to the established direction of the rivers. Cross-cutting and captures of westerly headwaters was the natural result of this change of slope.

The eastward tilt which the whole country got at the time the Rocky Mountains were elevated also affected the development of the main Loup. Without that upheaval the northern tributaries would have been dammed back by the silt along the Platte, and formed a series of swamps, instead of coalescing in a free-flowing stream.

That objection of Professor Davis, which is based upon the "systematic location" of Prairie Creek "between two parallel and larger rivers in a district of horizontal beds," is not serious. In the first place, I never dreamed of ascribing it to headwater erosion. It is obviously the result of Pliocene deposition crowding the Loup so far from the Platte that subsidiary drainage was developed on the intervening space. In the second place, this latest product, appearing upon the surface of a great mass of Pliocene silt, cuts no figure in determining the primitive course of channels lying at the bottom of that mass of silt.

Further criticisms from Professor Davis will be most welcome

L. E. HICKS.

The Aboriginal North American Tea.

IN *Science* for Jan. 22, 1892, is an abstract of Bulletin No. 14, United States Department of Agriculture, on "The Aboriginal North American Tea," *Ilex cassine*, which recalls to me that during our civil war, when the Confederate soldiers were encamped in the vicinity of the Rappahannock River, especially during the winter of 1862–3, that not only they, but also the inhabitants of that region, used freely the leaves of the American holly tree, *Ilex opaca*, in the preparation of a decoction as a substitute for China tea. This species of holly is not only abundant in that region, but grows to a large size, trees of eighteen inches in diameter and over being not uncommon in the thickets bordering the low grounds of the Rappahannock.

I do not know how they came to begin the use of this decoction, whether from a local handing down of the Indian custom of using the cassena tea, as Wood styles the *Ilex cassine*, or whether it may not have been suggested by soldiers from Alabama, who were numerous in the Confederate army, and who would be more likely to know of the use the Creeks made of the leaves of the shrub holly.

In this connection the question arises as to whether any use was made during our civil war of the leaves of the New Jersey tea, *Ceanothus Americanus*, which were used during the Revolution as a substitute for Chinese tea. JED. HOTCHKISS.

Staunton, Va., Feb. 24.

AMONG THE PUBLISHERS.

THE laboratory course in psychology, by Dr. E. C. Sanford, which is being published in parts in the *American Journal of Psychology*, is to be issued at a later date in book-form. It is the only practical course ever published.

— Messrs. J. Wiley & Sons, publishers of scientific works, New York City, have just issued the fourth edition of Thurston's "Manual of Steam-boilers," and the fourth edition of his "Friction and Lost Work in Machinery and Millwork." These works, like all others on their list, are kept under constant revision, and