

as such, but will appear in various forms: some of it may be evolved as heat, some as volume energy, some as kinetic energy, and it is even possible by an appropriate contrivance to obtain a large portion of the chemical as electrical energy. But to state that the energy always passes through the electric stage on its way to other forms in which it manifests itself to us is something altogether different.

The question that Prof. Armstrong tries to answer by the supposition that the presence of an electrolyte is required in order to bring about chemical change admits of a very different reply. We conceive it to be this: In most exothermic combinations the heat evolved is sufficient, provided the change were to proceed adiabatically, to resolve the compound into its constituents. Why, then, should they react? To take a concrete instance:—Why should ammonia and hydrochloric acid combine at ordinary temperatures when the heat evolved by their union is sufficient (*provided none escape*) to raise the reacting molecules to the temperature at which they refuse to combine? For convenience sake the question is stated in terms of heat, since that is the usual form in which the loss of chemical energy manifests itself to us; but it is advisable to keep the statement of the question quite general. It appears to us that the answer is:—because the reaction is not adiabatic. Some substances must be present—the walls of the containing vessel, some compound capable of dissociation, some solid body, such as spongy platinum, which will absorb a portion, perhaps an exceedingly small portion, of energy, and so give the bodies present a chance of interacting without liberating so much energy by their interaction as would decompose the prospective compound. These views, it may be contended, are speculative. It is true: but we venture to think that they are legitimate speculations, involving a complete survey of the circumstances, and not one-sided and partial like those of the paper we are criticising.

Assuming the correctness of Prof. Armstrong's main idea, there are still one or two matters of detail where the assumption scarcely seems in harmony with known facts. He assumes that because hydrogen chloride when dissolved in water forms a composite electrolyte, a gaseous mixture of hydrogen chloride and water will also be an electrolyte. This by no means follows, and indeed experiments which have been made in this direction point to the contrary conclusion. The same holds good of his argument as to the combination of nitric oxide and oxygen—water vapour is not known to form a composite electrolyte with gaseous nitric acid.

With regard to the regularity displayed by iodine and hydrogen compared with the irregularity of the results obtained by Victor Meyer with chlorine and hydrogen, it is altogether impossible to understand Prof. Armstrong's attitude. In one sentence he assures us that "this is not surprising," and in the next that "there is a significant [of what?] difference in the behaviour of the two mixtures, as hydrogen iodide should behave as hydrogen chloride." He suggests that some special electrolyte may be active in the case of chlorine and hydrogen; but he is inclined to account for the difference observed from the fact that only one of the reactions is reversible under the conditions of experiment. We quite fail to understand the influence which the reversibility of the reaction would exert on its regularity.

In fine, still assuming for the sake of argument the notion of "reversed electrolysis," we would ask:—In a mixture of hydrogen and oxygen, are the ions there, or are they not there? If not there, will the presence of a vapour bring them into existence? If there, what is the need of a so-called impurity? Is it supposed that the impurity will discharge them? Why, then, does not the presence of one or of two conducting wires of the same metal in an electrolyte cause combination of the ions?

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#### The Corona Spectrum.

IN the preliminary account by M. Deslandres of the main results of the eclipse photographs obtained by the French astronomers at Fundium, as reported in this journal on May 25 (vol. xlviii. p. 81), it is stated that many new coronal lines have been photographed, and that a displacement of the lines in the light from opposite points of the corona in the solar equatorial plane proves a rotational movement nearly corresponding with that of the surface of the sun itself.

In the absence of fuller details it is perhaps a little difficult

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to accept without reserve these interesting statements, particularly when one considers the somewhat unfavourable conditions under which the photographs were obtained. In the first place, one would like to ask by what means have these new bright lines been identified as belonging to the corona, seeing that, owing to the hazy condition of the air at the above station, the brilliant chromospheric radiations were apparently reflected from a considerable area of the sky in the sun's neighbourhood, forming, as it were, a kind of false corona with a bright line spectrum. So obvious, indeed, is this atmospheric spreading of the chromosphere lines in the spectrum photographs obtained by the English astronomers at the same station, that many lines are shown as clearly on the moon's disk as in the coronal regions; the calcium lines "H" and "K," which are very brilliant chromosphere lines, are in these found to extend considerably above the limits of the true corona, as defined by its continuous spectrum, and are also found equally bright across the dark moon.

From the above considerations one is inclined almost to doubt whether, after all, any true corona lines have ever been proved to exist, excepting perhaps the line 1474 (K), which is not ordinarily a brilliant line in the chromosphere, and would therefore not be easily seen by atmospheric reflexion;<sup>1</sup> and it would seem possible, if not probable, that this beautiful solar appendage, with its dark rifts and curving streamers, shines simply by continuous light.

Definite information on this point would, however, be gladly welcomed by those who are endeavouring to photograph the corona without an eclipse. We would, in fact, clutch at any straw, in the shape of a bright line, in the hope of its yielding a true image of the coronal forms, and it was hoped that the recent eclipse would furnish evidence which would settle this question.

With regard to the second point, namely, the displacement of lines in the coronal spectrum. This is said to be equal to a velocity in the line of sight of 5 to 7 kilometres per second (I presume for the total difference of position of the line), say 3 kilometres for the speed of approach or recession at a distance from the solar limb equal to two-thirds of the diameter.

This is certainly a very striking result, and if confirmed by further study would in itself go far to prove the true coronal nature of the line measured. A displacement is conceivable, it is true, under certain conditions, on the assumption that the light is reflected chromospheric light, but this would not exceed a velocity of 1.87 kilometres, whilst the above result comes not far short of an angular rotation equal to that of the disk itself. A point at the distance named would, if rigidly connected with the sun, alternately approach and recede at a speed of about 4.35 kilometres per second.

It would be interesting to know, however, what are the limits of error in these measurements. I gather that a high dispersion was not employed, and it would seem, therefore, that a large uncertainty may be expected; supposing, for instance, that in the original negative the lines H and K are depicted 25 mm. apart, the total displacement corresponding to 7 kilometres per second will only amount to 0.9 mm.; an error, therefore, of  $\frac{3}{10}$  mm., or  $\frac{1}{12.5}$  of an inch (corresponding to over  $1\frac{1}{2}$  kilometres) would materially affect the result; and to come within this limit would require unusually fine definition in the line measured.

In view of the novelty and great importance of the conclusions arrived at by the leader of the French eclipse expedition to Senegal, students of solar physics will await with keen interest, not to say impatience, the publication of a full detailed discussion of the results obtained.

J. EVERSHED.  
Kenley, Surrey, July 2.

#### Lord Coleridge and Vivisection.

MY attention has been called to a letter which the Lord Chief Justice has written in support of an endeavour which is being made by a section of the Society for Promoting Christian Knowledge to withdraw from circulation my little work "Our Secret Friends and Foes," recently published in their "Romance of Science Series." Until the Publication Committee of the

<sup>1</sup> It seems pretty certain, however, from the clearly-defined coronal "rings" seen by Prof. Lockyer and others at former eclipses by means of an objective prism, that a more or less uniform gaseous extension must exist far above the chromosphere and prominences; but is this the corona proper?