

From this number and from the protein content of the muscle, it is possible to calculate that the total surface of all the protein of 1 gm. of muscle would be ± 50 sq. m., assuming that all the protein is in the same condition as the protein on the water surface (a highly improbable supposition). Now lactic acid—like other acids—has a striking influence on the size of the area occupied by this protein solution on a water surface. After adding so much lactic acid that the pH changes gradually from 6.4 to 4, we find that the area remains practically constant from 6.4 to 5.4 and changes suddenly between 5.4 and 4.8 into one having much smaller dimensions. At about pH 4 it is not much larger than 450×10^{-16} sq. m.

There is a close agreement between the small quantity of lactic acid required to bring about this sudden change of the spreading of the protein molecules, and the lactic acid produced in a single contraction as calculated by A. V. Hill (0.031 mg. per gm. muscle). Much depends on the buffer value of the solution used for the experiments. If we take a very feebly buffered solution of $\frac{1}{1000}$ mol. KH_2PO_4 , so little as ± 1.6 c.c. of a tenth molar solution of lactic acid per litre is capable of producing the effect (0.015 mg. per c.c.).

From our experiments with hæmoglobin (*Proc. Roy. Acad. of Sci., Amsterdam*, Dec. 19, 1925) and with other proteins, we are inclined to consider this change in the size of the protein molecule on a water surface under the influence of differences of the pH of the solution as a general property of all proteins. We only add here that further increase of the acidity tends to produce the reverse effect, so much so, that the largest surfaces occupied by protein molecules are two to four times larger than those on neutral water. We get on $\frac{1}{10}$ molar hydrochloric acid a film having a thickness of 6.6 to 7.5×10^{-8} cm. These are considered by us as layers having the thickness of one amino-acid. (Cf. de Nouy, *Journ. Biol. Chemistry*, 64, 1925.)

There exists some evidence indicating that hæmoglobin is orientated at some surface inside a red blood cell in such a way that all the hæmochromogen and iron is placed at the surface. By analogy we suggest that the cystine (or glutathione) part of each protein molecule in muscle is orientated at some external surface, and it is probable that this surface is again the surface of the fibrils, and that the glutathione-lectithin is orientated in opposite places, in order to give the reversible reactions of reduction and oxidation, during muscular contraction and relaxation.

E. GORTER.

F. GRENDL.

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Bigamous Hydrogen—a Protest.

NATURE is always both helpful and challenging and though these be degenerate days, in which the policeman stops duelling, as there is no organised scientific police force, some of us must answer her call and at least constitute ourselves special constables in protection of the liberties of our craft. Scientific son as I am, plus sixty years old, of Frankland, who was the first to enunciate the doctrine of valency, upon which our entire system of structural chemistry is based, I have been accustomed always to regard hydrogen as the most single-minded and moral of monogamists. If the story of chemistry, especially that of paraffinic chemistry, mean anything, it is that carbon and hydrogen are mutually satisfied when the carbon atom is married with four hydrogen atoms. The indifference to all external temptation of such unions is most remarkable: whether rightly or wrongly, we have, in

consequence, built our entire symbolic edifice upon the assumption that hydrogen is a consistent monad.

I notice that, in the lecture, in honour of Biot, which Prof. Lowry gave recently, in Paris, upon his own work (*NATURE*, February 20, p. 271), he brought forward certain freak formulæ for tartaric acid, in which hydrogen figures as bigamist. I publicly protested against such aspersion by him of its character, last April, in Brussels: yet the libel is repeated. I may say, he but follows the loose example set by certain Uesanians, especially one E. N. Lewis, a Californian thermodynamiter, who has chosen to disregard the fundamental canons of chemistry—for no obvious reason other than that of indulging in premature speculation upon electrons as the cause of valency: in effect, he has but substituted pairs of dots for the chemist's single bond, thereby deluding himself, apparently, into the belief, that a love-pair of electrons may be hugged by the single hydrogen atom. The way in which the speculation is set out is open to question chemically on almost every page. To quote one case: we are told, that having painted hydrogen as electronegative, as the analogue of fluorine (!), the author had predicted that metallic hydrides would prove to have the character of salts and give hydrogen at the anode. This prediction, we are told, "has been entirely verified in the work of Bardwell (1922), who succeeded in electrolysing a melt containing calcium hydride and obtaining hydrogen at the anode, in amount corresponding to Faraday's Law." No proof has been given, that this hydrogen was not merely displaced by the alkali-metal liberated, during electrolysis, from the fused mixture of alkali-metal chlorides with the hydride used in the experiment. That a writer who belongs to the physical school should be oblivious of such possibility is astounding.

The bigamy of hydrogen is disguised by the blessed word co-ordination—which to-day covers a multitude of wild speculations. Hydrogen, like oxygen, forms co-ordination compounds, says Prof. Lowry. "The real existence of this type of co-ordination can be proved [*sic*] both by crystallographic and by chemical evidence. The X-ray analysis of ice has proved [*sic*] that each atom of hydrogen is surrounded symmetrically by four atoms of hydrogen and that each atom of oxygen is placed symmetrically between two atoms of oxygen."

I heard Sir William Bragg give his paper on ice at the Physical Society and well remember how carefully he pointed out, that the experimental evidence was in no way complete. Do we in the least know the structure of ice? Does position, as determined by X-ray analysis, settle valency? Sodium in salt has six chlorines and chlorine six sodiums surrounding it—are sodium and chlorine to be declared hexads? We are assured that they are not even combined in salt. I would add—we do not believe this, those few of us who are chemists and take some notice of laboratory facts.

As to Prof. Lowry's chemical evidence—

"The simplest chemical evidence for the co-ordination of hydrogen is to be found in potassium hydrofluoride, $\text{KHF}_2 \rightleftharpoons \overset{+}{\text{K}} + \overset{-}{\text{HF}_2}$. A complex anion is here formed by the union of two fluoride-ions and one hydrogen-ion. Since the fluoride-ions are both negatively charged and each ion carries a complete octet of electrons, there can be no direct attraction between them. Since, however, both fluoride-ions have an attraction for the positively charged hydrogen-ion, this may very well act as a link between them as in the formula: $\overset{-}{\text{F}} : \text{H} : \overset{-}{\text{F}}$ or $\overset{-}{\text{F}} \overset{+}{\text{H}} \overset{-}{\text{F}}$, where the two

fluorines are linked together by a hydrogen nucleus, in just the same way as the oxygen atoms in ice."

From beginning to end this passage is nothing but stark, unsupported and unsupported assumption and assertion. As it is "off the rails," such speculation, however, in these days of loose thinking, is regarded as something to be worshipped and imitated. "Dragons that are but clay within and brass without" still secure adoration. As of old, they need to be burst asunder with "lumps of seethed pitch and fat and hair."

Prof. Lowry finds difficulty in "the free rotation of the molecule [of tartaric acid] about the single bond between the two central carbon atoms"—hence his device of connecting by means of hydrogen.¹ I suggest that there is no sufficient evidence that such free rotation is usual, if possible. He also speculates as to the character of the dissymmetry of camphor. On this, may I suggest, that the camphor formula which we are accustomed to use is geometrically impossible. I began my career by doubting Kekulé's formula—at its close, I take leave to question the long fashionable substitute and would even assert that the views now current upon the structure of the compounds of the polymethylene and terpene class are often fundamentally unsound and in need of revision.

Prof. Lowry's early optical work is of such perfection, that it is most unfortunate that he, of all chemists, should now indulge in these obscurantisms. The bump of judgment seems to be lacking in the modern school of chemistry—the judicial sense is nowhere to be found. Seemingly, any wild-cat speculation may be indulged in upon paper. Surely, it were time that we returned to serious work in the laboratory and that some effort were made to inculcate the principles of logic into the minds of university students. The entire system of academic teaching is fast becoming suspect: the product is not what we need in practice: it has no power of reasoning upon observation.

HENRY E. ARMSTRONG.

New Experimental Results concerning the Doublet $K\beta_1$.

IN a paper recently published (*Z. f. Phys.* 33, 601, 1925) the present authors came to the conclusion that the new line β' constitutes with the line β_1 the "relativity doublet." In a letter received by the authors, Prof. Sommerfeld points out that, in his opinion, the line β' ought to be considered as a spark line or as a line of unknown origin, since $\Delta\lambda$ according to our data is equal to $4.3 X.U.$, whilst according to Allison's $\Delta\lambda$ for M_0 is equal to $0.56 X.U.$ The value given by Allison may be verified with the aid of the data for the wave-lengths $L\beta_3$ and $L\beta_4$ which are emitted, when the transition of electrons takes place between the levels M_3 or M_4 and the level L_3 , while the transition of electrons from the same levels M_3 and M_4 to the level K gives rise to the lines $K\beta_1$ and $K\beta'$. Accordingly we obtain for $\Delta\nu/R$ from the data for the L series for M_0 the value 1.63, and from Allison's data -1.27.

The agreement is quite satisfactory. However, we cannot act similarly in our case, as the data for the L series for manganese and the neighbouring elements are not known. Nevertheless, Sommerfeld's point of view may be subjected to another test. In accordance with Wentzel's theory the presence of the spark

¹ In discussing the anomalous rotatory dispersive power of solution of tartaric acid, in 1913, Dr. E. E. Walker and I suggested alternative formulæ (*N. Soc. Proc.*, 88, 388). We need to know if any change in real acidity take place as the acid changes. Perhaps, as crystal, it may be the dicarboxy-acid and pass over in solution into the monocarboxy-maleic (phthalic) form. Prof. Lowry puts chemical considerations aside.

lines in the part of the spectrum close to the line $K\beta_1$ is due to the fact that the atoms of elements from copper downwards, possessing an uncompleted group M , readily pick up an additional electron and retain it on one of the M levels. Consequently the levels in the corresponding atoms ought to be displaced, and therefore a displacement of the lines of the K series must be expected.

What is the actual process by means of which the catching of an additional electron in the uncompleted group takes place? This may be realised in different ways. For example, the electron striking the anticathode may under favourable conditions penetrate inside the atom and be kept there in the group M , or one of the electrons ejected from the level K may also be stopped in the group M . There are undoubtedly other possible cases, when the electrons may be caught in the uncompleted group. Therefore the existence of atoms of two different kinds seems to be probable: the normal atoms and the atoms charged with an additional electron in the uncompleted M group, by means of various processes.

It is obvious that the atoms in which the additional electron has been ejected from the K level possess the greatest probability of the emission of the line $K\beta'$. If this process actually takes place, we are justified in expecting to find in the absorption spectrum the absorption lines of the wave-lengths equal to $K\beta'$, or close to it. The effect of the absorption may be easily estimated. The relative intensities of the lines of the K series are 100, 50, 35, 17.5, and 15 (Siegbahn; Seljakow and Krasnikow). Hence, it may be stated that the probability of the transition of electrons from the M level to the K level will be roughly 8 per cent. of the probability of the ejection of electrons from the K level. Therefore an electron must be ejected from the K level to one of the M levels not less than 8 times in 100, in other cases being expelled outside the atom. From Glocker's formula it may be calculated that a line in the absorption band, after passing through a sheet of iron or manganese 0.02 mm. thick, will be weakened 1000 times more than any line out of the absorption band under the same conditions. Then one can expect that the lines $K\beta_1$ and $K\beta'$ from manganese and iron, after passing through sheets of manganese and iron 0.02 mm. thick, will suffer weakening to a markedly different degree, or at least that a new absorption line will appear close to the line $K\beta'$.

The experiment gives opposite results: namely, the ratio of the intensities of the lines $K\beta_1$ and $K\beta'$ in the absorption spectrum remains actually the same as in the emission spectrum. There is also no trace of any absorption line in the region we are interested in. Moreover, it was of some interest to know if the line $K\beta'$ would appear at the critical voltage or not. For that purpose the constant voltage was supplied by a Siemens machine of 10,000 V. Thus the voltage did not exceed the critical value for manganese by more than 200-300 volts. Experiments have shown that, in the presence of the line $K\beta_1$, the line $K\beta'$ was always present, and vice versa, in the absence of the first line the second was also absent. The ratio of the intensities remained unaltered and equal to that observed at high voltages.

On the strength of these experiments combined we are led to conclude that the level from which the transition of an electron gives rise to the line $K\beta'$, may be considered as a normal, and hence the combination of the lines $K\beta_1$ and $K\beta'$ is a "relativity doublet." Ray, Ray and Siegbahn (*Phil. Mag.* 1, 1924, and *Phys. Ber.*, 1925) have shown that the law $\Delta\lambda = \text{const.}$ does not hold for the "relativity doublets" in the region of elements, where, according to Bohr, the reconstruction of the inner electronic groups takes