tain amount of nitrogen fixed as acid. He created entirely new nitrogenous compounds, among them calcium cyanimide. He used pressure of hundreds of atmospheres to force nitrogen and hydrogen into combination in the form of ammonia. He looked the old-fashioned coke-oven over, and, horrified at its waste of valuable nitrogen, proceeded to devise a retort which would yield ammonium sulphate as a by-product.

Now Chile and the London Stock Exchange must take account of his work. The production of synthetic ammonia in 1926 seems to have been equivalent to 650,000 tons of pure nitrogen—more than twice that of Chile. Had it not been for the British coal strike and the depression in the iron and coke industries, an additional 340,000 tons would have been obtained as sulphate of ammonia. As it is, by-product ovens accounted for 240,000 tons of that form of nitrogen. Chile must at least reduce her export tax, and the companies that exploit her greatest natural resource must engage a few first-class research chemists to devise more economical methods of treating the nitrate scooped from the earth. What research has destroyed research may also save.

The chemist may be pardoned if he smiles as he reads the "prices current" for Chilean and synthetic nitrates and notes that there is more than a full year's visible export stock of Chilean nitrate to be disposed of and scarcely no stock at all of synthetic nitrate. For years he has been dinning the gospel of research into the ears of bankers and manufacturers. Now that his synthetic ammonia has broken a monopoly to which every nation long paid tribute, his audience is larger and more attentive. We actually believe him when he assures us that he can make gasoline in a factory and sell it in competition with natural motor-fuel, or that some day he will make synthetic rubber so cheaply that we can pave streets with it.—*The New York Times.*

DISCUSSION AND CORRESPONDENCE

ILLINIUM

IN a copy of *Gaz. chim. Ital.* (*56,* 862 (1926)) received a few days ago, Professor Rolla, of Florence, claims priority for the discovery of Element No. 61 and proposes for it the name Florentium on the basis of a "Plico Suggellato" filed in June, 1924. Professor Rolla began his search for the element early in 1922; see *Z. anorg. allgem. Chem., 157,* 571 (1926). In making his claim for priority he was, apparently, not aware of the following facts:

In 1919 the University of Illinois and the U. S. Bureau of Standards entered on a joint investigation of the arc spectra of rare earth elements, using materials resulting from long continued fractionations carried out at the University of Illinois. The results of this investigation were published in the U. S. Bureau of Standards Scientific Papers, *421* (1921), *442* (1922), *466* (1923). In the second of these papers, published at about the time that Professor Rolla began his work and two years before his "Plico Suggellato" was deposited, Dr. Kiess, who carried out the spectrometric studies, reported 130 spectral lines which were common to the spectra of Neodymium and Samarium, in the samples submitted to him by Professor Hopkins, and says, "These lines are of unknown origin and may belong to the missing element of order No. 61, coming between Neodymium and Samarium." In January, 1924, again five months before the deposit of Professor Rolla's document, L. F. Yntema published an article, "Observations on Rare Earths. XV. A Search for Element 61," in which he gives five additional lines in the ultra violet region and repeats the statement that these probably belong to Element No. 61. See J. Amer. Chem. Soc., *46,* 37 (1924). Finally, on the basis of still further work, including the finding of two X-ray lines of the L series, J. A. Harris with B. S. Hopkins announced the discovery of Element 61 and proposed the name Illinium. See J. Amer. Chem. Soc., *48,* 1594 (1926).

In the light of these facts, it would seem that the honor for the discovery of No. 61 belongs primarily to Professor Hopkins and that the element should be called Illinium rather than Florentium. This does not detract from the credit which Professor Rolla should receive for his independent discovery of the element. Both Professor Rolla and Professor Hopkins realize that a very large amount of additional work must be done before the element can be fully accepted.

W. A. NOYES

URBANA, ILL.,
JAN. 29, 1927

CONCERNING THE RING METHOD FOR MEASURING SURFACE TENSION

WHEN looking over the literature of the past five years, the writer can not refrain from being highly gratified by the large number of papers published on the ring method for measuring surface tension. Indeed, he can not help but feel that he is responsible to a certain extent for this sudden interest in a very old method as, previous to his first paper describing his instrument (a combination of the ring and of the torsion balance, 1919), so little attention had been given to the technique of the ring that hardly two or three workers had used it in twenty years. A few of the recent articles, however, are critical and tend to establish the inaccuracy and the unreliability of the method which he advocates. Some of them, signed by the best authorities on surface tension, are of great interest, but, as they strongly emphasize the shortcomings of the *instrument* itself, now on the market, they are likely to throw discredit on the *method* which we maintain to be the best for the study of colloids. Therefore, the writer thought it necessary to add a few words to this discussion. It has never been his intention to claim that the dimensions which he chose for the stock platinum ring were the ideal ones, for *all* kinds of work. No stock instrument can claim so much, not even stalagmometers and glass tips. For standard work of the highest accuracy, the glass tips have to be carefully calibrated and ground by the experimenter, and are not on the market. The same applies to capillary tubes. For very small values of surface tension, it is advisable to use tips of a different size than those used for water and certain aqueous solutions. This is true of practically every physical apparatus. There is no doubt that a knife-edge ring, such as is used by Dr. Klopsteg and myself in certain careful measurements, is better than the ordinary stock platinum ring with a circumference length of 4 cm. But the tensiometer was made principally to determine *very rapidly* the surface tension of a small quantity of liquid with accuracy and was particularly intended for the study of the time effect on aqueous colloidal solutions; now, the values obtained for pure water are in excellent accord with those accepted as standards, from which they differ by less than ± 0.1 dyne. The agreement is better than that which is to be found in the data published by different authors using drop weight methods. This was considered as satisfactory. Dr. Johlin (Science, 1926, lxiv, 93), acknowledges the fact and explains it by stating that the "approximately correct (?) values found with the ring supplied with the instrument are the result of the cancellation of equal and opposite errors." This is indeed a great compliment to the instrument, in fact the greatest that can be made to any instrument. Further, he states that the value obtained for benzene is too high. Probably he considers the data obtained with the capillary ascension method as the absolute standards. But this method is known to give lower values than the others, and has been seriously criticized by a number of excellent authorities, A. Ferguson among others. In the tables, the surface tension of ethyl alcohol is given as 22 dynes at 20° C. (Ramsay and Shields, capillary ascension), but Grunmach found 26.1 dynes at 17.7° C. (capillary waves), and Freundlich ("Capillary Chemistry," 3d ed., p. 43 of the English translation) quotes 21.6 dynes at 25° C. These values do not agree. When a liquid is in contact with its vapor, the readings are different from those obtained when it is in contact with air. As long as no absolute theoretical values of the surface tension of pure liquids are available, it is impossible to condemn a method because, under certain conditions, in the case of certain liquids, it does not agree with another.

In addition, I have lately read with great satisfaction a letter by Professor Harkins in *Nature,* in which he states that he and his collaborators have worked out a correction formula for the ring method reducing the errors to one per cent., in all cases, and that they hope to reduce them eventually to one tenth of one per cent. Such a statement issued by one of the greatest authorities on surface tension ought to settle the question definitely.

Dr. Johlin, in his paper in Science, evidently aiming to correct the writer, says that "two hours can not be assumed as sufficient for reaching the state of even approximate equilibrium. Frequently the change following an initial period of two hours is several times as great as it was in this initial period." I feel sure that Dr. Johlin will give me credit for not having overlooked such a possibility and that he has understood, as I have, that the time necessary to reach an equilibrium is function of the concentration, of the mobility of the molecules or particles in solution and of the distance they have to travel to reach an adsorbing surface. The latter condition may be expressed by the value of the ratio $\frac{\text{Surface}}{\text{Volume}}$, the importance of which has been emphasized in my book. A stable value is attained in less than two hours when 2 cc of a sodium oleate solution at concentrations between 1/25,000 and 1/1,000,000 are contained in watch-glasses; 100 cc of the same solutions will require at least sixty-four hours to reach their equilibrium when placed in a petri dish 10 cm in diameter (see "Surface Equilibrium of Biological and Organic Colloids," p. 174).

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THE BASIS OF REFLEX COORDINATION

In Science, Vol. LXIV, No. 1650, A. Forbes has raised some objections against my theory of specific accord between the excitations sent off by the central nervous system and the motor end-organs. My theory is based on two main points:

(1) On the phenomenon discovered by me[1,2], and since confirmed by G. Hertwig,[3] Detwiler[4] and W.

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[1] P. Weiss, Arch. f. mikrosk., Anat. u. Entwicklg. mech., Bd. 102 (635)—1924.

[2] P. Weiss, *Jour. Comp. Neur.,* Vol. 40 ( )—1926.

[3] G. Hertwig, Sitzungsber. d. natur-forschd. Gesellsch. Rostock, Vol. 1, 192.

[4] S. R. Detwiler, *Jour. Comp. Neur.,* Vol. 38 (461)—1925.