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 $\pm$ 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.

[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.

Brandt,[5] that in a supernumerary transplanted limb, when innervated from the limb level of the spinal cord, every muscle enters into action, always at the same time and with the same degree of intensity as does the homologous muscle in the normal limb close to it. It is not quite correct to state, in respect to this phenomenon, as Forbes does, that "the nature of the reflex coordination involved is best illustrated by the fact that in movements of progression all flexor muscles contract together, while the extensors relax, and *vice versa,*" for it is easy to evoke experimentally others than progression reflexes, where not all muscles which are synergic in progression work together; in this case, as well as in the supernumerary limb, not all flexors or extensors are found to exhibit contraction at the same time, but only those among them which are homologous to the normal muscles at work.

(2) On the fact that in innervating the transplanted limb the outgrowing nerve fibers during their course are dividing each in several branches, their subsequent distribution being entirely a matter of chance, as there is not any specificity involved in directing the single nerve fibers. So at least the great majority, if not all, of the motor ganglion cells innervating the supernumerary limb have their several peripheral branches ending on muscles of different kinds.

Forbes admits that if this be really the case my statements in respect to "some power to select a special component in excitation" in the muscle would be correct. But he continues: "Weiss furnishes neither proof nor evidence for his assertion that a single motor neurone may innervate antagonistic muscles. . . . The individual spinal root, containing many axons, may so branch as to supply both the normal and the supernumerary limb, but the individual axon may (and probably does) remain unbranched till it approaches the muscle and there distributes itself only to adjacent fibers."

In reality, I did furnish such proofs for my assertion. Every one can see by an exhaustive study of my paper of 1924 that Forbes's conception just mentioned is by no means in accord with the facts or with my statements about these facts. In reconstructing in three animals with supernumerary transplanted limbs the nerve paths, I found and described that the individual axon *does not* remain unbranched till it approaches the muscle. What really happens is, on the contrary, that the nerve fibers cut off by implanting the limb *branch immediately after beginning their outgrowth* and are *widely distributed long before entering the nerve paths* of the limb to be innervated by them. The fiber branches, in running through the pathless scar, do not at all remain together and when reaching the proximal end of the transplanted limb are so confused that, save in exceptional cases, the order of the fibers in entering the different nerve channels of the transplant is quite other than it was in leaving the central nerve stump. So it is clearly seen that it is quite incorrect to believe the fibers to augment only when they have reached the muscle, as Forbes does. The augmentation takes place long before.

In overlooking this point, it may be easy to give an interpretation of the observed phenomenon on the basis of the classical nervous physiology and there would not be any need to accept my theory. However, recognizing the haphazard disorder of the outgrowing and dividing fibers, as proved by my microscopical examinations, and as recorded in my paper of 1924, we are obliged to accept a resonance-like mechanism involved in the nervous action on the muscle system.

The statement of the older theory that coordination of muscular action is determined within the central nervous system remains untouched by my theory. Only one point must be changed; whereas, after the former theory, the central coordinations were believed to depend on a geometrical distribution of excitation on the paths connected with the muscles to be brought to work at the given moment, it consists, in the new theory, of a dynamical selection of specific excitation forms adapted to the different muscles (the selection may perhaps consist in the excitation of centers which produce discharges just of these forms).

A resonance theory in such a general form as I proposed is the only explanation I can think of which in all respects is in concordance with the facts observed. To bring it in accord with the opinions of nerve physiology generally held will be a matter of future investigation. There are, it is true, many discordances; especially, as is pointed out by Forbes, there is a striking incompatibility between a resonance theory and an all-or-none-principle. But, is the all-or-none principle one of normal reflex action? It may be, as is Forbes's opinion, that there is no definite proof *against* the assumption that this principle would hold good not only for the inadequate stimulation of the nerve itself, but also for the adequate central innervation. I may point out, however, that, on the other hand, there is no convincing evidence or proof to *confirm* this assumption.

For all further information I may refer to an extensive publication of my theory which will appear in a few months.

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[5] W. Brandt, Arch. f. mikr., An. u. Entw. mech., Vol. 106 (193)—1925.