ART. XXXI.—*The Compression of Powdered Solids: A Note by W. SPRING.*

MR. HALLOCK, in a note inserted in this Journal,* has been good enough to recognize that his article entitled "The Flow of Solids" had through oversight been worded in such a way as to attribute to me the exact opposite of the conclusions I had drawn from my researches. In effect, whereas my experiments were made in view of determining whether certain characteristic properties of the liquid state, such as the faculty of welding together, etc., existed to a more or less minimized extent in solid bodies; Mr. Hallock, on the contrary, thought that the manifestation of these properties required previous liquefaction, so that I should have been wrong in assigning to the solid state some of the characteristic properties of liquids. The experiments he has made to verify this point have shown him that matter does really remain solid under strong compression, unless the bodies experimented with occupy, as is the case with ice, a smaller volume in the liquid than in the solid state. But in the latter part of his note Mr. Hallock again takes up the interpretation of my results and objects that the majority of the phenomena mentioned by me are not due to cubic compression, but rather to kneading under pressure.

* Vol. xxxvi, p. 59

I regret to be obliged to state that this new objection of Mr. Hallock's falls wide of its mark, like the first; this time it is a case of misunderstanding fortified by confusion, for he again repeats exactly what I have always asserted myself. I may, therefore, be allowed to explain in English this time, on account of the importance of the subject, and to avoid a recurrence of misinterpretations, which simply mean a loss of time and labor to everyone concerned.

When ten years ago* I began my researches on the compression of powders my sole object was to ascertain whether solid bodies possessed the property of welding when their particles are put in perfect contact. I desired to find out whether the phenomenon of regelation could be generalized, since Faraday had shown that contact, or an extremely slight pressure, suffices for its production, contrary to J. Thomson's view, who said pressure was the cause of the phenomenon. To realize this contact I thought it would be sufficient to press the bodies strongly together. To my mind, moreover, pressure was not an active agent in the matter, but only a means to the end, and I looked for the effects to contact alone. I have expressed myself thus on this point in nearly all my articles and it will therefore be sufficient to quote the following.

After having recalled† the fact that certain bodies, such as sodium nitrate, become a coherent mass when their powders are left undisturbed in a bottle for a certain space of time, and that the coherence of the mass depended upon contact, I said, "now, to increase the number of points of contact in a powdered body, it will be sufficient to put it under pressure heavy enough to cause the spaces between the fragments of the body to become filled up with their debris."

In another place‡ I said with regard to chemical reaction produced in my experiments—"one must not not lose sight of the fact that pressure is not a chemical agent to the same extent as heat or electricity." But as I have always thought that *contact* was brought about by compression I have often for the sake of brevity, spoken of "*welding due to pressure*" instead of always saying "*welding due to contact produced by compression*." I now see that I was unwise in thus wishing to economize my time.

Besides, as conclusive proof that it is always to *contact* that I assigned welding phenomena, chemical reactions and also in part the diffusion of solids, there is the fact that I deemed it necessary to operate *in vacuo*, on account of failures in preliminary experiments made under the ordinary conditions. This is

* Bulletin de l'Académie de Belgique, II, xlv, 1878.

† Bulletin de l'Académie de Belgique, II, xlix, p. 336, 1880.

‡ Bulletin de la Société chimique de Paris, vol. xli, p. 497, 1884.

what I said on this point in 1880,*—"so that in my preliminary experiments I had not operated *in vacuo* and must consequently have enclosed a notable quantity of air in the powders I compressed; this, besides preventing by its presence intimate contact between the particles of solid matter and hindering their union, etc."

If a different pressure is necessary for different bodies, it is solely because all bodies are not equally hard and not because pressure is the immediate cause of their welding.

Better yet, in all articles I have hitherto published on the chemical reactions of powdered solids I have shown the influence of time on the chemical phenomena, by leaving the compressed bodies undisturbed for more or less time, and out of the compressor. Here it is evident that pressure had nothing more to do, as it existed no longer. Quite recently I sent an article to the *Zeitschrift für Physik u. Chemie*, containing the results of experiments of this kind, which lasted four years, and showing how a chemical reaction can complete itself if the matter is allowed the time for diffusion. They also explain Mr. Hallock's results on the formation of alloys.

Finally, from a rational point of view, there is no reason for assigning an active part to pressure in the phenomenon of welding of bodies pure and simple, for the act of welding by no means brings about a permanent decrease in the volume of the matter, and from that moment it is not conceivable how it would possibly act otherwise than to establish as perfect a state of contact as possible. When Mr. H. states that welding takes place better by "motion under pressure" he is quite correct, for then the sliding of the grains on one another is eminently adapted to bring about perfect contact, because it expels interposed air and moisture, exactly as when clay is kneaded with the fingers.

We will now consider another view of the question, and examine the phenomena in which pressure acts as such, i. e. by the diminution in volume it causes in the matter operated on.

In my experiments published in 1880 I found that pressure brings about changes of allotropic state in bodies, when the least dense variety is compressed at a certain temperature. This is what pressure does. Many proofs now exist in favor of my conclusions. I will confine myself to quoting the fine researches of van't Hoff and Reicher on the shifting of the transition point by pressure.† These works have proved, in theory and practice, that rhombic sulphur passes to the monosymmetric variety at 95.6° at the ordinary pressure, and at 96.2° under 12 atm.; in other words 12 atm. pressure shift the point of transition 0.6° . This being the case 1,552 atm.

* Loc. cit.

† *Études de dynamique chimique*, Amsterdam, 1884, p. 198.

would transform one variety of sulphur to the other at a temperature of 18° . So it is not very astonishing that I arrived at this result with 6,000 atm. pressure, and I acquired this fact four years prior to the works of van't Hoff and Reicher.

Again, pressure intervenes as such to *facilitate phenomena of solid diffusion*, when the volume of the matter after diffusion is smaller than that before diffusion. This I have already proved by numerous experiments, and will do so again in some I have already had in hand for a long time.

Mr. Hallock says on the subject of solid diffusion, "I think no one would expect that a piece of copper and tin soldered together would diffuse and form a homogeneous bronze, nor would they expect that simple cubic static pressure would promote these reactions." To my mind Mr. H.'s opinion would be valid if the resulting bronze were less dense than the mixture of copper and tin, but in the opposite case there would be found, not a homogeneous bronze, but an interpenetration of the copper and tin at the surface of perfect contact, which would cease when the difference in density of successive infinitely thin layers, no longer had too high a value. This I have proved for the reaction between Na_2CO_3 and BaSO_4 ,—and conversely—by studying the influence of time on the phenomenon and by showing that after some weeks a state of equilibrium is set up between the two bodies.*

I am aware that these results may appear extraordinary, but I must express myself categorically, as they represent facts. I will also add that on his side Mr. C. Roberts Austen† has obtained the blue alloy of copper and antimony at the surface of contact of the metals under pressure.

I have neither exhausted my reply nor the subject, but I will let things remain *in statu quo* at present, as I think I have sufficiently shown that I never attributed an active part to pressure in the welding of bodies, but that it intervenes as such only when a decrease in volume is necessary to produce a physical or chemical change of state at a given temperature, and also because I shall treat all those points in detail when uniting my results in book-form, after the termination of my experiments.

* Société chimique de Paris, 1884 et 1885.

† From a letter of Mr. Austen to the author.