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complex tellurohydrogen sulphides. The investigation of this exceedingly complicated subject is being continued in the direction indicated.

§ 12. Summarizing the results of these investigations, I believe I may say:

1. That the elements tellurium and sulphur do not form compounds, but mixed crystals.

2. That the elements tellurium, selenium and sulphur behave in quite an analogous manner towards the sulphides of the alkali and alkali-earth metals, and cause the formation of complex sulpho-, seleno- or tellurohydrogen sulphides of a different type, and that it is quite unnecessary to presuppose the intermediate formation of selenium-sulphur or tellurium-sulphur compounds.

3. That the position, given by DUMAS, to tellurium in the sulphur group as the first homologue of selenium is quite justified so far as the mutual behaviour of these elements is concerned, and that sulphur, selenium, and tellurium form a natural triad of elements, which are more adjacent to each other than any one of them is to oxygen.

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**Physics.** — “*Some remarks on Prof. KOHNSTAMM’s reply.*” By Dr. J. J. VAN LAAR. Communicated by Prof. LORENTZ.

In these Proceedings of Jan. 6<sup>th</sup> 1910 Prof. KOHNSTAMM has inserted a reply to my remarks suggested by a paper by Messrs. TIMMERMANS and KOHNSTAMM. Though I, too, very reluctantly continue the discussion, I feel obliged to briefly revert to this matter for the last time, in order to prevent further misunderstanding.

So I will just point out that Mr. KOHNSTAMM is quite silent about the *cardinal* point of my remarks, given in point *a* second part, point *e* and point *f*; viz. that Messrs. T. and K. in consequence of their disregard of the last five of my seven papers on the subject in question have wrongly asserted that the “abnormal” type III could not occur for *normal* substances, and that this would be due to my restricting supposition  $a_{12} = \sqrt{a_1 a_2}$ . Only to remove this misunderstanding — as I had asserted the very opposite of this — I wrote my preceding paper.

On the other hand a few minor questions are extensively discussed in the answer, viz. the question  $a_{12} = \sqrt{a_1 a_2}$  and  $\frac{d^2 b}{dx^2} = 0$ . I must remark here that when I repeatedly spoke of the "quite general" case  $a_2 \lesseqgtr a_1$ ,  $b_2 \lesseqgtr b_1$ , this expression "quite general" was obviously meant in *contrast* to the special case  $a_2 \lesseqgtr a_1$ ,  $b_2 = b_1$ , treated by me before in the first two Papers, as would be clear to everybody, and that the "generality" meant by me according to the whole tenour of my papers, of course, only holds *within the area of the once assumed supposition*  $a_{12} = \sqrt{a_1 a_2}$  (BERTHELOT'S). For this supposition I explicitly premised in all my papers, and I repeated it more than once (*loc. cit.*).

Now with regard to the question itself of the supposition  $a_{12} = \sqrt{a_1 a_2}$  (which, however, is not the point at issue), I may be allowed to remind Mr. K. of a paper of his in the *Zeitschr. f. physik.* Ch. 36 p. 41 (1901), where it, *inter alia*, says at the end (p. 62): "So weit ich aus dem mir bekannten Material zu schlieszen vermag, scheinen mir jedoch die Tatsachen sehr zu Gunsten der (BERTHELOT'SCHE) Annahme zu sprechen..." I will add that I, too, consider the supposition  $a_{12} = \sqrt{a_1 a_2}$  as very probable, and that seeming deviations from this supposition are attributed by me to the formation of compounds. But I hope to treat this more fully on a later occasion.

I now demonstrated that even on this simple supposition the abnormal type III can occur for *perfectly normal* substances. And this Mr. K. denied — as my later papers on this subject in which this was proved by me, had evidently escaped his notice.

With regard to the supposition  $\frac{d^2 b}{dx^2} = 0$ , Mr. K. refers to my statement that "qualitatively everything will remain the same if  $b$  is not assumed independent of  $v$  and  $T$ ". This, however, is quite beside the question whether the supposition  $\frac{d^2 b}{dx^2} = 0$  is of influence on my results or not; for  $v$  and  $T$  are not the same thing as  $x$ . I fully maintain my contention, and Mr. K. will, no doubt, understand, that this dependence on  $v$  and  $T$  was only mentioned by me, because VAN DER WAALS' later investigations have shown that  $b$  still depends on this quantity. But this is not the point in question.

I, however, readily acknowledge that when writing the lines about the longitudinal plait closing again, quoted by Mr. K., I did

not sufficiently clearly state that the divergent result was only founded on the assumption  $\frac{d^2b}{dx^2} = 0$ . I knew, however, that VAN DER WAALS in his *Continuität* II p. 24 has already treated this question. Yet on theoretical considerations I abide by my opinion that in the neighbourhood of the limiting volume, so at very high pressures,  $\frac{d^2b}{dx^2}$  must be  $= 0$ .

And now I think that I for my part, have sufficiently elucidated Mr. KOHNSTAMM's Reply, so that further misunderstanding seems almost precluded.

Baarn, Febr. 21, 1910.

**Mathematics.** — “*The oscillations about a position of equilibrium where a simple linear relation exists between the frequencies of the principal vibrations.*” (1<sup>st</sup> part). By Mr. H. J. E. BETH. (Communicated by Prof. KORTWEG).

*Introduction.*

§ 1. In his paper<sup>1)</sup>: “On certain vibrations of higher order of abnormal intensity (vibrations of relation) for mechanisms with more degrees of freedom” (*Verhandelingen der Koninklijke Akademie van Wetenschappen*, Vol. V. N<sup>o</sup>. 8, 1897; *Archives Néerlandaises* Vol. I, series II, pages 229—260) Prof. KORTWEG has written down the expansions in series for the principal coordinates of an arbitrary mechanism with more degrees of freedom, performing small oscillations about a position of stable equilibrium. From these expansions in series could be deduced that in a certain case it was possible for some vibrations of higher order, having in general a small intensity with respect to the principal vibrations, to obtain an abnormally great intensity; this is the case when between the frequencies  $n$ ,  $n_y$  etc. of the principal vibrations a relation exists of the form

$$pn_x + qn_y + \dots = \varrho;$$

where  $p$ ,  $q$  etc. are positive or negative integers and  $\varrho$  is with respect to  $n_x$ ,  $n_y$  etc. a small quantity, called residue of relation.

Furthermore however it became evident that, when  $S \leq 4$  ( $S$  is the sum of the absolute values of  $p$ ,  $q$  etc.) and at the same time  $\varrho = 0$ ,

<sup>1)</sup> “Over zekere trillingen van hooger orde van abnormale intensiteit (relatie-trillingen) bij mechanismen met meerdere graden van vrijheid”.