

*Citation:*

Kohnstamm, Ph., A short reply to mr. van Laar remarks, in:  
KNAW, Proceedings, 12, 1909-1910, Amsterdam, 1910, pp. 534-537

In brackets after the figures of both series is given the place number which each of the genera would take in a regular classification. A comparison of these place numbers shows at a glance in how far the position and the slope of the For. magn. go hand in hand. In general there appears to be a decided parallelism between these features in monkeys, and only in a few cases there is a fairly marked difference between position and slope. This is, for instance, the case in *Chrysotrux* where the angle is small in comparison to the position, and in *Colobus* where the reverse is the case.

At the beginning of this paper mention was made of the opinion held by HUXLEY, viz. that the slope of the For. magn. is in proportion to the degree of prognathism. In a following communication, which will deal with the prognathism of the primate skull, this view will be discussed at greater length.

**Physics.** — “*A short reply to Mr. VAN LAAR's remarks.*” By Prof. PH. KOHNSTAMM. (Communicated by Prof. J. D. VAN DER WAALS).

In the proceedings of the preceding meeting of this Academy Mr. VAN LAAR made some remarks suggested by a paper by Mr. TIMMERMANS and me. Though these remarks do not call in question in any point the validity of our results, but exclusively deal with the question whether we have done sufficient justice to the share Mr. VAN LAAR has had in the construction of the theory, I think that both politeness to Mr. VAN LAAR and deference to the communicator of these remarks forbid me to leave them unanswered. So I shall try to state as shortly as possible the reasons why I still think I have done full justice to that share.

1. Mr. VAN LAAR writes in point *a* of his remarks:<sup>1)</sup> “Here I must remark that I have *never*”<sup>2)</sup> represented the special case  $a_{12} = \sqrt{a_1 a_2}$  as the general case.”

In writing this Mr. VAN LAAR had certainly forgotten that he wrote in These Proc. Sept. 1906 p. 227: “In the *third* paper in These Proceedings (June 24, 1905) the equation:

$$\Delta = \frac{1}{T_1} \left( \frac{dT}{dx} \right)_0 = \theta \sqrt{\frac{1}{\pi}} \left[ \theta \sqrt{\frac{1}{\pi}} \left( \frac{3}{2} - \frac{1}{2} \sqrt{\frac{1}{\pi}} \right)^2 - 1 \right] \dots (3)$$

was derived . . . for the *quite general*<sup>2)</sup> case  $a_2 \leq a_1$   $b_2 \leq b_1$ ”, etc.

And on the same page: “Now the restricting supposition  $\beta = 0$

<sup>1)</sup> These Proc. XII p. 455.

<sup>2)</sup> Mr. VAN LAAR's italics.

was relinquished for the determination of the *double point* of the plaitpoint line, and the *quite general case*<sup>1)</sup>  $a_2 \geq a_1$ ,  $b_2 \leq b_1$  was considered.

And on p. 228: "We can, namely, characterize *all possible pairs*<sup>1)</sup> of substances by the values of  $\theta$  and  $\pi$ , and finally it will *only*<sup>1)</sup> depend on *these values*,<sup>1)</sup> which of the three main types will appear."

And on p. 230: "The calculations get, however, so exceedingly intricate that they proved practically unfeasible for the *general case*<sup>1)</sup>  $a_2 \geq a_1$ ,  $b_2 \leq b_1$ ."

And on p. 231: "This appears already from the fact that the substitution of the *quite general assumption*<sup>1)</sup>  $b_1 \leq b_2$  for the simplified assumption  $b_1 = b_2$  has made no change in the existence of a double point . . . , and that also the calculations for the limits of type III . . . may be carried out for the *quite general case*<sup>1)</sup>  $b_1 \geq b_2$ ."

And on p. 232: "The calculation proves that in the *quite general case*<sup>1)</sup>  $b_1 \leq b_2$ " etc.

For, everywhere where the *general case* is spoken of here, it is the case  $a^2_{12} = a_1 a_2$  that is meant, and also the quotation from p. 228 is possible only, by an identification of the general case and this special one.

2. In point *b* of his remarks Mr. VAN LAAR says in connection with our sentence that his investigations: "very onesidedly, lay the stress on the existence of open plaits, a circumstance which by no means can be considered as a *result*<sup>2)</sup>, as it immediately follows from the arbitrary, if not erroneous *supposition*<sup>2)</sup> of the linear dependence of  $b$  and  $x$ ": "Now I have never asserted that  $\frac{d^2b}{dx^2} = 0$  would always agree with what actually happens; again I have simply *assumed*<sup>3)</sup> this in order to make the *calculations*<sup>3)</sup> possible."

Yet I read on p. 231 of the cited paper: "We shall once more emphatically point out that the *numeric*<sup>3)</sup> results of our investigation will naturally be modified, when  $b$  is not assumed to be independent of  $v$  and  $T$  . . . but that *qualitatively*<sup>3)</sup> everything will remain unchanged."

And on p. 233: "Then further increase of pressure makes the phases 1 and 2 again diverge . . . without the longitudinal plait ever closing again — *as was formerly considered possible*<sup>1)</sup> — [cf.

<sup>1)</sup> The italics are mine.

<sup>2)</sup> T. and K's italics.

<sup>3)</sup> Mr. VAN LAAR's italics.

inter alia VAN DER WAALS, Cont. II p. 190 (1900)]. Only at temperatures higher than  $T_0$  . . . there can be question of homogeneity to the highest pressures."

It seems to me that every unprejudiced reader of these lines must acknowledge that Mr. VAN LAAR thought that he gave a new *result* here, materially differing from the result of a closed plait as it was thought possible by VAN DER WAALS, and that he cannot possibly have realized when writing these lines that this divergent result was only founded on his assumption  $\frac{d^2b}{dx^2} = 0$ .

3. As to point *c*, the sentence mentioned there really refers to a paper by Mr. VAN LAAR earlier than April 1905 (viz. of January 1905). I did not know, however, until the publication of the "Remarks", (and now I only know it from these "Remarks") that Mr. VAN LAAR has abandoned his views of this previous paper. Else we should, of course, not have mentioned it.

4. With regard to point *d* we must protect Mr. VAN LAAR against himself. We had said: "His results are of importance particularly because they showed that under certain circumstances non-miscibility can occur for perfectly normal substances, a fact which was generally doubted at the time." Mr. VAN LAAR remarks in this connection that it was by no means generally doubted up to now whether miscibility could occur for normal substances but only whether some special "abnormal" forms of non-miscibility could occur for perfectly normal substances. I must maintain in opposition to this that both LEHFELDT and VAN DER WAALS, to whom we referred i.e., had by no means a special case of non-miscibility in view, but very decidedly all non-miscibility. So Mr. VAN LAAR's merit is decidedly greater than he will own here. On the other hand I must confess that in our endeavours to be perfectly objective to Mr. VAN LAAR, we have really got unjust in the above cited sentence to Mr. VAN LAAR's predecessors: VAN DER WAALS and KORTEWEG. The above statement might lead one to think that Mr. VAN LAAR had been the first to demonstrate the possibility of non-miscibility for normal substances. As Mr. VAN LAAR justly remarks: this is incorrect, and it would have been better if our sentence had run like this: His results are of importance particularly because he adhered to the possibility of non-miscibility for normal substances in a time in which this was pretty generally doubted, and showed once more that for certain values of  $\alpha$ 's and  $b$ 's, which could not a priori be considered as improbable, non-miscibility must really appear".

If I wanted to discuss also Mr. VAN LAAR's other remarks, I should

have to enter fully into the very heart of the matter, as I cannot assume the reader to be fully acquainted with the details of these investigations. But then I should think I abused the hospitality which this Academy so courteously extends in its publications also to non-members. So I think that the above will suffice. If Mr. VAN LAAR should, however, wish to pursue this discussion elsewhere, I am willing, though not desirous, to continue it.

**Chemistry.** — “*The equilibrium solid-liquid-gas in binary systems which present mixed crystals.*” By Dr. H. R. KRUYT. (Communicated by Prof. P. VAN ROMBURGH.) First communication.

In the Archives Néerlandaises [2] 5 (Jubilee number in honour of Prof. LORENTZ) p. 360 (1900) Prof. BAKHUIS ROOZEBOOM published an article “Sur l'équilibre de cristaux mixtes avec la phase vapeur” in which he described and illustrated the  $p-t-x$  surface of a binary system when exclusively homogeneous mixed crystals occur as a solid phase. He treats the case of unlimited miscibility in all phases and especially for a system in which the melting point line proceeds without a maximum or a minimum. He has, moreover, limited himself to the case that the three-phase line solid-liquid-gas (*SLG*) also occurs without a maximum or a minimum.

These matters have not been further investigated theoretically<sup>1)</sup>; there was in fact no inducement to do so, as there has been an almost entire absence of experimental research. Only two investigators, SPERANSKI<sup>2)</sup> and KÜSTER<sup>3)</sup> furnished material as to the equilibrium of mixed crystals with a gas-phase, whereas the researches of HOLLMAN<sup>4)</sup> belong to a category of more complicated phenomena.

I intend to carry out a series of investigations in order to extend our knowledge of the systems showing a miscibility in the solid condition. First of all, I will accept the facts already known and, therefore will discuss at present, *theoretically*, the various possibilities of the progressive change of the three-phase line indicated by ROOZEBOOM (l.c.) and communicate later the results of an *investigation*

<sup>1)</sup> The results obtained by A. SMITS (Proc. (1908) XI p. 165, and Zeitschr. f. physikal. Chem. (1909) 67, 464) do not differ from those of ROOZEBOOM. The only paper I know connected with this subject is a communication of MEYERHOPFER: “Ueber Reifkurven”, Zeitschr. f. physikal. Chem. 46, 379 (1903).

<sup>2)</sup> Zeitschr. f. physikal. Chem. 46, 70 (1903) and 51, 45 (1905).

<sup>3)</sup> Ibid. 51, 222 (1905).

<sup>4)</sup> Ibid. 37, 193 (1901),