referring to the abstract of M. Konovaloff's paper in the *Journal* of the Chemical Society for January, 1886, I meet with the following:—"As an explanation of this contact-action phenomenon it is asked whether it is not possible that the bombardment of the molecules on the solid matter causes the kinetic energy of the molecules to be transformed in part into the internal work required for their decomposition."

Perhaps some of your correspondents will kindly furnish me with references to original memoirs (or other sources of information) in which I may find this question competently treated. The idea here put forward by M. Konovaloff is surely not new. It might be extended, as I conceive, to such cases as, for example, the combination of SO_2 and O_2 to form SO_3, the formation of ammonia from a mixture of NO and H_2 in the proportion of $5H_2 + 2NO$, the formation of NO from a mixture of NH_3 with an excess of O_2, in each case when the gaseous mixture is passed over heated platinum-sponge or platinised asbestos. For some years past this explanation of such contact-action phenomena has appeared to me much more reasonable than such explanations as are generally suggested. The high temperature required in such cases seems to point rather to something in common with the initial dissociation caused by the intense heat of the electric spark, when oxy-hydrogen gas is fired. In such cases as those referred to above the lesser intensity of the heat applied from without may easily be compensated by intra-molecular results of the increased energy with which the impact of individual molecules must take place at high temperatures, and the great extension of the heated solid surface exposed to their bombardment. Under this view (with which my pupils have been familiar for some years past) combination is brought about through the atoms of some of the molecules of the mixed gases being brought into the *quasi-nascent state.* A. IRVING

Wellington College, March 10

Variable Stars

IN NATURE for March 11 (p. 440) Dr. Mills, in criticising Prof. Seeliger's *collision hypothesis* of the blazing forth of *Nova*, advances a theory of his own as a presumably original and novel explanation of the phenomena of variable stars. It may be of interest, therefore, to point out that practically the same explanation was suggested in 1878 by Prof. R. Meldola in a paper published in the *Philosophical Magazine* for July of that year.

In this paper the author states: "It is conceivable that in certain cases the composition of a star's atmosphere may be such as to permit a considerable amount of cooling before any combination takes place among its constituents; under such circumstances a sudden catastrophe might mark the period of combination, and a star of feeble light would blaze forth suddenly, as occurred in 1866 to τ Coronæ Borealis. In other cases, again, it is possible that the composition of a star's atmosphere may be of such a nature as to lead to a state of periodically unstable chemical equilibrium; that is to say, during a certain period combination may be going on with the accompanying evolution of heat, till at length dissociation again begins to take place. In this manner the phenomena of many variable stars may perhaps be accounted for."

It will be seen that these hypotheses are essentially identical, although it would appear that Dr. Mills limits his explanation to the formation of polymerides (presumably of some primordial matter), these constituting our chemical "elements." I cannot see, however, that he has any reasons for excluding the formation of true compounds, or why he should consider a variable star as necessarily one that is engaged *only* in "making elements." This last process would, no doubt, be the first to take place on the hypothesis of cooling from a state of complete dissociation, but there would surely come a period when the more stable chemical "compounds" could exist, and their formation would also be attended by the evolution of heat and possibly of light also. JNO. CASTELL-EVANS

London, March 13

The Iridescent Clouds and their Height

COL. TENNANT is mistaken in supposing that the only peculiarity of the clouds which appeared in December 1884 and 1885 is in their being fringed with coloured spectra, though these were, I believe, much more vivid than those of ordinary clouds, as described by him; besides which, my impression was that the colours were more varied than is usually the case. Col. Tennant, with his experience, will be better able to say than I am whether there is generally as much blue in the clouds he describes as in those under discussion. I stated in my letter of Dec. 29 last (p. 199) that there was no special amount of blue in the clouds seen the previous day, but on the 31st there was a good deal. However, I do not insist on this as being any important difference; but, by referring to the numerous letters this year and last about the clouds, he will see there were several other characteristic points.

These clouds are not like any ordinary clouds; if they can be referred to any of the usual classes they are cirrus, but decidedly different from any cirrus we generally see. Their usually very smooth texture was striking, though some on December 28 (1885) had the ordinary appearance of rippling, but in most cases this was too slight to be visible without optical aid, even when the clouds were broken up into narrow wisps, and in such a position that no colour was produced there was still something in their appearance which struck me as different from ordinary clouds. The frequently rectangular shape was very singular also, though they had not always this form. I said I had not observed this shape in the clouds of December 28, but other observers noted it on that occasion (see pp. 219, 220), and on the 31st I saw many of the clouds with this outline. It is shown indistinctly and with the corners cut off in Mr. C. Davison's sketches (pp. 292, 293). The form is generally described in the letters you have published as rhomboidal, but this is an effect of perspective; no doubt if the clouds were seen overhead they would appear rectangular. Their great height, too, must have been unusual, though perhaps not greater than that of the singular coloured clouds seen last summer in Bavaria by myself and in this country by others, as described in NATURE, and which differed from the clouds I am now describing in some important particulars. One patch of cloud was observed both here and at Shields on December 28, and a calculation from a comparison of the position as seen from the two places gives its height as 23 miles; while making the utmost allowance that seems permitted for the roughness of the observations only reduces its elevation to 11 miles. That it was the same patch of cloud observed from both places is undoubted, for one observer of it (H. R. Procter) was travelling from Shields to Sunderland, and he saw that it was the same patch all the time, and the one I had been observing here. The fringes of colour were distinctly visible on this cloud up to 4h. 25m., and feebly so till 4h. $27\frac{3}{4}$m. I concluded that the sun had not ceased shining upon it till that time; if so, its height would be between 11 and 12 miles. At 4h. $28\frac{1}{2}$m. it was pink with sunset colouring; but the sun need not have been shining on it then.

The iridescent colours have no connection with halos, as supposed by Mr. Stone (p. 391), no particular colour appearing at any particular distance from the sun, but every colour being seen at any distance, though more vividly at perhaps from 15° to 30° off the sun. THOS. W. BACKHOUSE

Sunderland, March 12

Forms of Ice

A CURIOUS formation has lately occurred on the surface of a sheet of ice in a tub. Being under a tap, the ice became submerged below several inches of water. Fresh ice then formed as thin vertical plates upon, and at right angles to the submerged sheet. These plates meeting each other in all directions, produced a spongy mass, 3 or 4 inches thick. I do not know if it is a common production, but the special interest attached to it is that it would seem to suggest how "spongy" quartz has arisen, of which I have a specimen consisting of thin and nearly parallel plates; as well as the well-known form of thin crystalline plates in which calcite may occur. It is just this form of calcite which gives rise to "hacked" quartz, when silica has coagulated or crystallised over a mass of such thin crystals, and then these latter have been subsequently dissolved out.

Why a sheet of ice should increase regularly in thickness by additions to its lower surface, and form this spongy mass on its upper, is a question I should like to hear solved.

Another form of ice I lately noticed on a wall consisted of minute prisms standing in little depressions in the bricks. The circumference of the prism partook of the irregular form of the cavity, giving the appearance of an upward growth.

While speaking of ice, I should like to venture a suggestion to account for its lighter specific gravity than that of water, namely, that water crystallises in macles of complex form; the