etc., which have chiefly occupied my attention, and it appeared, at first, as if the question of polyploidy scarcely arose at all; even now, after the investigation of more than sixty species, only seven have been found to be other than diploid. Among these, however, one, *Silene ciliata*, has shown itself of such exceptional interest that it seemed to me worth while directing special attention to it. In the genus Silene, although more than thirty species have been examined, only two show polyploidy: of these *S. vallesia* is a normal tetraploid, whilst in the case of *S. ciliata* I have investigated two races, one of which is tetraploid and the other has a haploid count of 96 and is therefore 16-ploid. The plants of this latter race were perfectly normal in chromosome behaviour and fertile. The two races were obtained from different sources and both were quite typical, and this within a species which is somewhat variable.

Although cases are known in which tetraploidy does not produce a 'gigas' form, it does seem somewhat surprising that a race can arise with four times the normal number of chromosomes, and with actually the largest but one recorded figure for angiosperms, and yet show no obvious difference in appearance.

That this is a case of continuous duplication of chromosomes rather than segmentation of the individual chromosomes is strongly suggested by the observation that the size of the chromosomes is little less in the case of the 16-ploid race; even the characteristic ring shape at the heterotype division is retained. The volume of the pollen mother-cell in the two forms is very little different, that of the tetraploid being about three-quarters that of the 16-ploid. The chief difference in cytological appearance is due to the extremely close packing of the chromosomes in the latter case.

This record seems to minimise the value which has frequently been attached to mere multiplication of chromosome sets and, at any rate within the limits of the Silenoideæ, to emphasise the importance of the original set characteristic of the genus.

KATHLEEN B. BLACKBURN.
Armstrong College (University of Durham),
Newcastle-upon-Tyne, July 4.

Optical Behaviour of Protein Solutions.

A VERY remarkable increase in light-scattering power is exhibited by gelatine solutions when the hydrogen-ion concentration approaches the value (about $p\mathrm{H} = 5$) corresponding to the iso-electric point. This effect, which appears to have been known for some time, has been recently studied in detail by Kraemer and his co-workers, who give interesting curves showing the manner in which the Tyndall effect varies with $p\mathrm{H}$ and temperature ("Colloid Symposium Monograph," vol. 4, and *Journal of Physical Chemistry*, May 1927).

The phenomena are scarcely intelligible on the view commonly adopted that the Tyndall effect in a colloidal solution is simply proportional to the number of scattering particles of the same kind present in it. Their explanation becomes clearer if we apply to colloidal solutions the general thermodynamic theory of light scattering, in which the Tyndall effect is regarded as due to local fluctuations of optical density in the medium. According to the latter theory, the scattering power of a colloidal solution would be connected with the osmotic pressure P of the particles, by the relation

$$\frac{\pi^2 RT}{2N\lambda^4} \frac{k(\partial\epsilon/\partial k)^2 \rho/m}{\partial P/\partial k}, \quad . \quad . \quad . \quad (1)$$

where k is the concentration of the dispersed material, ϵ is the optical dielectric constant of the solution and ρ/m is practically unity for a dilute solution. It is well known from the work of Jacques Loeb that the osmotic pressure of a gelatine solution alters in a notable manner with $p\mathrm{H}$, becoming very small at the iso-electric point. Equation (1) then enables us to see at once why the Tyndall effect becomes very large under the same conditions.

A detailed discussion of colloidal optics on the basis of the thermodynamic theory of light scattering will be published in the *Indian Journal of Physics*.

C. V. RAMAN.

The Relationship between Chinese and Arabic Alchemy.

SINCE my communication on the subject of Chinese alchemy (NATURE, Jan. 1, 1927) was written, I have received the very important memoir entitled "Chemistry in Iraq and Persia in the Tenth Century A.D.," by Principal Stapleton, the late Mr. Azo, and Mr. M. H. Husain, published in the *Memoirs of the Asiatic Society of Bengal*, vol. 8, No. 6, 1927. In this (p. 405) the treatise on Taoist alchemy, *Pao p'o tsz* of Ko-Hung (A.D. 330), to which I referred, is mentioned, and a summary of the alchemical contents of three of the 'Inner chapters' (*Nuy peen*), translated from an edition published in 1884, is given. This information does not go further than that already published, and it would seem that the account given by Edkins is nearly complete. The authors then remark (p. 406) that: "The above account is sufficient to prove (*a*) that Chinese alchemy was concerned almost entirely with Mineral Chemistry: and (*b*) that Ko-Hung's materials were so extraordinarily similar to those used by Arabic and Greek alchemists that it is certain that Chinese, Greek, and Arabic alchemy must have had a common source of origin." They also refer (p. 405) to "a further possible source of Arabic alchemy, namely, the Chinese School of Alchemy which was flourishing at least as early as 200 B.C."

It will be seen that the distinguished Oriental scholar has reached the same conclusion as myself: the only difference between us appears to be that he regards Chinese alchemy of 200 B.C. as well established, whilst I consider that this still awaits satisfactory demonstration. An examination of the information dealing with this earliest period by a competent expert is still necessary.

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Fictitious Amazons.

THE efforts of syndicated journalism to popularise research occasionally lead to misunderstandings. I have to-day received a cutting from the *Daily Mail* of Bombay, dated June 14, devoting two columns to discussion of views about the ancient Amazons, which I am represented as having maintained at a meeting of the British Association for the Advancement of Science. Will you give me the opportunity of saying that neither before the British Association nor before any other public body have I discussed the ancient Amazons at all. Similar announcements have been appearing in French, Belgian, and especially in American, papers during the past six months, in connexion with the Philadelphia meeting of the American Association last December. It is true that I attended that meeting as the representative of the British Association, and gave a public lecture on some aspects of the geography of Greek lands; but that lecture contained no reference to the Amazons.

Will Indian, French, Belgian, and especially American newspapers 'please copy'?

JOHN L. MYRES.
New College, Oxford, July 19.