A New Synthesis of X-Ray Data for Crystal Analysis

IT is unfortunate that Dr. Yü is not more easily accessible to direct correspondence about his new synthesis of X-ray data for the determination of crystal structures[1]. If the method is what he claims it to be, it is certainly an important advance; the details he gives, however, are not self-explanatory, and physical reasoning would suggest that the advantages he claims cannot possibly be obtained from the X-ray data given by any actual crystal.

The number of X-ray spectra obtainable from a crystal is limited by the natural decrease in intensity with angle; thus there is a limit to the resolving power of the 'X-ray microscope'. This limit is not, as for optical instruments, an artificial one that depends on the conditions of experiment; it is an expression of the fact that atoms have a natural diffuseness due to their finite size and thermal motion. Crystals composed of dimensionless atoms would give spectra that did not, on the average, decrease with angle.

Now Yü has claimed, in effect, that he has produced a synthesis with no limit to the resolving power. To do this he produces a set of quantities E_h which are related to the observed intensities but which are so corrected that they do not, on the average, decrease with angle. This, in agreement with my statement above, is the condition necessary for infinite resolving power. These E_h's are then multiplied by the quantities $a_{\mu h}$; Yü does not define these but states that they decrease with h so that his series $\sum_{h=0}^{\infty} a_{\mu h} E_h$ can be terminated at a reasonable value of h. It seems to me, however, that if the high-order intensities are not included in the series, the resolving power cannot be infinite; that the convergence is in the a's and not in the E's is immaterial.

The method, of course, cannot be completely understood until all the details are available. Nevertheless, I suspect that there is no essential difference between Yü's synthesis and Patterson's, except that Yü considers his as a function that has appreciable values only at discrete points, whereas Patterson's is continuous. It is doubtful whether this concept has any advantages over the older one, but it will be interesting to see whether the publication of Yü's $a_{\mu h}$'s will lead to any simplification of the processes involved in the summations.

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[1] Yü, NATURE, 149, 638 (1942).

A Relativity Query

As is well known, Einstein's theory of gravitation predicts three small effects not previously known. Two of these are the deflexion of light rays passing near the sun and the advance of Mercury's perihelion. I have found that these are given very simply by Fermat's principle and the principle of least action respectively.

The matter can be put shortly, as follows:

$$\delta \int \sqrt{1 + \left(r\frac{d\theta}{dr}\right)^2}\, dr = 0, \qquad (1)$$

an application of Fermat's principle, gives the straight line; r and θ are two-dimensional polar co-ordinates.

$$\delta \int \sqrt{\frac{2\,km}{r} - C}\,\sqrt{1 + \left(r\frac{d\theta}{dr}\right)^2}\, dr = 0, \qquad (2)$$

an application of least action, gives the ellipse. Now repeat the two equations but suppose that the medium is very slightly ælotropic, writing

$$\sqrt{n_r^2 + \left(n_\theta\, r\frac{d\theta}{dr}\right)^2}\, dr$$

for the element of optical length where

$$n_r = n_\theta^2 = \left(1 - \frac{2km}{rc^2}\right)^{-1}$$

and c is the velocity of light. The application of Fermat's principle then becomes

$$\delta \int \sqrt{n_r^2 + \left(n_\theta\, r\frac{d\theta}{dr}\right)^2}\, dr = 0; \qquad (3)$$

and the application of least action

$$\delta \int \sqrt{\frac{2km}{r} - C}\,\sqrt{n_r^2 + \left(n_\theta\, r\frac{d\theta}{dr}\right)^2}\, dr = 0 \qquad (4)$$

The two equations give respectively *exactly* the same paths as Einstein obtains for the passage of the light ray and the planet in the sun's gravitational field. The results come out very easily. It is the slight ælotropy of the medium that constitutes the difference between Newton and Einstein. The first three equations can be obtained from the final one by omitting the appropriate terms.

Now these seem to be important results, and there is a strong probability that they have appeared in print before, but I have not been able myself to find a reference to this having happened, neither have authorities whom I have consulted. Can any reader of NATURE give me information?

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Geologists in War-Time

MIGHT I suggest that the lack of interest shown in the work of geologists in war-time, complained of by Dr. F. Coles Phillips in NATURE of April 4 last, is a continuation of a similar lack of interest in peace-time? This subject was dealt with ably by Prof. P. G. H. Boswell in his presidential address to the Geological Society of London last year, and it is now being realized that, admirable and fundamental as pure science may be, any branch of science which is not linked to everyday life is bound to suffer the fate that is being suffered to-day by pure geology. Mining and oil geology have already robbed the science of many potential adherents, but what has struck me most since my interest in geology was aroused over twenty years ago has been its complete neglect of its possible application to allied sciences. Thus to take only those cases of which I have personal knowledge, building stones, road and building aggregates, and greater than these, soil mechanics, are all examples of subjects which could have been developed by geologists, but which have been left by them to chemists, physicists and engineers with markedly beneficial results to those branches of science and a corresponding loss to geology.

It should be realized by geologists that the natural corollary to the statement made above is that a greater interest in allied sciences by them is needed; the impetus must come from them; it will certainly not come to them from other quarters.

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