others, have not, however, followed Dana—a crystal right-hand crystallographically, is also right-hand optically with them.

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I AM much indebted to Prof. Cheshire for stating so clearly the historic incidence of the confusion which has arisen in regard to the designation of the two types of optical rotation, as to which shall be called right-handed and which left-handed, due largely to the reversal of Biot's convention by Sir John Herschel, and to the similar reversion in the second edition of Dr. Pereira's book. Other investigators and experimenters have also adopted the reversal, for instance, Sir William Spottiswoode at the time he was president of the Royal Society, for on pp. 47-48 of his book, "Polarisation of Light," we read: "A right-handed ray is one in which, to a person looking in the direction in which the light is moving, the plane of vibration appears turned in the same sense as the hands of a watch." Moreover, if instead of using the polariscope as a table instrument one projects the phenomena on the screen, the picture there displayed is reversed exactly like a lantern slide, which has to be inverted in the lantern (the two spots in front at the top being brought to the bottom at the back), in order to get an upright picture on the screen. Thus, for example, in the mica-sector experiment of the late Prof. S. P. Thompson (pp. 1103-1104 of the second edition of my "Crystallography and Practical Crystal Measurement"), the black cross moves on the screen one sector to the left for a right-handed quartz crystal and to the right for a left-handed one; whereas on looking through the same instrument used as a table polariscope the movement is to the right for a right-handed crystal, in accordance with the Biot convention.

It is thus important to know the exact conditions of the experiment whenever the question of the correct discrimination of right- or left-handedness in the optical rotation of crystals is being dealt with. Further, the safest course, in the case of quartz, is to cut the section-plate to be used to afford the definite decision from a crystal which is clearly a single individual, and not a twin, showing the little s and κ faces unmistakably, and this course was pursued by me in the preparation of my "Crystallography." As most in accordance with current practice (that of von Groth and Pockels, for example), and in rightful deference to Biot, the discoverer of the two optically active kinds of quartz, Biot's convention was used, in both editions of the book, a course which it is satisfactory to learn meets with the approval of Prof. Cheshire. The apparent opposite, on p. 1101, lines 7-8, is due to this being a projection experiment, the observer looking towards the screen along with the light rays; the direction here, however, really does not matter, as only the colour of the centre of the field is being referred to; even here, perhaps, it would be better in any future edition (they were not present in the first edition) to omit the words "from the point of view of the observer looking in the same direction as the light is being propagated," the text then conforming clearly with the Biot convention. On p. 1083, where a table experiment is being referred to, and the conditions are otherwise similar to those in the centre of the field in the case just referred to, there is no ambiguity, the Biot convention being clearly followed.

It was felt desirable to direct attention to the confusion which has so obviously arisen, and this was done in the footnote to p. 1082. Emphasis was attempted to be laid on the fact that, after all, the phenomena are due to the passage of the ray through the helical structure of the crystal, now so happily confirmed by Sir William Bragg's X-ray analysis of quartz, and that the observer does well to imagine himself travelling with the rays through the screw, in order to appreciate the cause of the rotation of the plane of polarisation or vibration of the light rays. The particular screw type, right- or left-handed, is the same, however, whether we regard the screw from one end of it or the other, whether we look along with or against the light stream; otherwise it would matter which side up the quartz plate were arranged, that is, which side were placed the nearer to any particular one of the nicols. But the optical effect, the rotation of the plane of polarisation or of vibration of the light rays, is, of course, what matters and what is so clearly different for the two different types of quartz helices, and it was my intention to retain and use the Biot convention for its directional (right- or left-handed) designation. The footnote in question is not sufficiently explicit, and must be amended in any future edition. Prof. Cheshire and the writer are, however, quite agreed on the facts, and that this Biot convention shall be the one employed, and I am grateful to Prof. Cheshire and to the editor of NATURE for affording me the opportunity of stating this.

A. E. H. TUTTON.

Space-Time Geodesics.

IN his letter in NATURE of November 25, replying to mine which appeared in NATURE of October 28, Prof. Piaggio points out that the equations of Space-Time geodesics may be deduced by other methods than those of the calculus of variations, and suggests that, in some such way, it is possible to get over the difficulties to which I directed attention.

My criticism, however, was directed, not merely against the definition of Space-Time geodesics as *minimum lines*, but against all seeming definitions of them which start from ideas of measurement as a fundamental basis.

I must, however, in passing, warn my readers against what at first sight looks like a suggestion, though I have no doubt that it was not so intended by Prof. Piaggio, that Space-Time geodesics might be defined in terms of "*the osculating plane*."

If there were any strict analogy with the case of geodesics on surfaces in ordinary three-dimensional geometry, such an "*osculating plane*" would (apart from a line of intersection) have to lie in some mysterious region outside our Space-Time continuum altogether. Prof. Piaggio, however, I have no doubt, wishes to lay stress upon the equations he obtains.

I was of course aware that the *equations* of Space-Time geodesics could be arrived at by various analytical devices; but how much better off does this leave us? Consider, for example, the simple Space-Time analysis as given by Minkowski and see what it implies.

In the first place, it implies a set of co-ordinate axes x , y , z , and t , which are themselves *geodesics*.

How are these particular geodesics to be defined?

They cannot be defined as minimum lines, for they are not minimum lines; and we cannot use our co-ordinate system to define them, since we are now contemplating how the co-ordinate system can be set up.