unit, or, in other words, is the number of identical or enantiomorphously related asymmetric parts into which it is subdivisible, if m is the number of molecules it contains and p the symmetry number of each molecule, then $n = mp$. Mr. Barker believes that Fedorov failed to prove his case, that the first paper referred to above contains an unconscious repetition of Fedorov's argument, which, though new evidence is brought forward, is still unconvincing, and that the suggested structure for tartaric acid is against the principle and not, as we have said, in its favour.

In the first place, Fedorov's statement was surely unexceptionable in the form in which he made it. If one of the molecules, or groups of molecules into which the unit is divided, possesses a plane of symmetry, this can mean only that it has similar relations with its neighbours on either side of the plane and through them with the rest of the crystal. That is to say, the plane of symmetry of the molecule is also a plane of symmetry of the crystal. On the other hand, we must be ready to allow, as Sir William Bragg has pointed out, that a molecule as built into a crystal may not have the same form as the freer molecule of a liquid or a gas. Such a difference seems to occur in the case of tartaric acid, on which account the crystal and its solution differ in their optical properties. The molecule may have a plane of symmetry in one case and not in the other. It is a task of the future to correlate the forms and the symmetries of the molecule in its different conditions. It is by no means improbable that the differences are small (Journ. Chem. Soc., 1922, vol. 121, p. 2766). Fedorov was quite aware of this possibility himself. If Fedorov's statement is taken to refer to the molecule as built into the crystal, it seems to require no further defence.

In the next place, the rules or principles set out in the first of the two papers referred to do contain Fedorov's statement, no doubt. If the author had been aware of the paper he would have referred to it. But the essence of the statement which is criticised is not an enunciation of a law of crystal symmetry which could not have been and was not overlooked by the searching examination of the crystallographers. It was an attempt to codify certain results of X-ray analysis. Fedorov could say, rightly as we think, that a crystal of the monoclinic prismatic class could be formed of four groups, A, B, C, and D: of which B was obtained from A by reflection across a plane, C by digonal rotation about an axis, and D by inversion through a centre of symmetry. He had no direct evidence to carry him further. The X-rays do go further: they show that in the crystal unit of benzoic acid, for example, there really are four groups so related to one another, and they give their relative positions. Moreover, they show that each of these groups is, in substance at least, the chemical molecule. This is new knowledge, which could not be proved by Fedorov. If it had been in his power to do so, the crystallographic tables would have contained the dimensions of the unit cell of each crystal; and not merely, as they do now, the topical ratios.

We may point out that Mr. Barker is in error also in supposing that nothing can be said about the symmetry of the molecule until the position of every atom in it is accurately determined. The X-rays show that the molecules of benzoic acid, for example, are divisible into two groups, which present exactly the same aspect when viewed along the axis of the crystal and different aspects when viewed in any other direction. This is in agreement with the hypothesis that the two are the reflections of each other across the plane of symmetry, and that each is by itself asymmetric with respect to that plane.

Lastly, Mr. Barker refers to the structure of tartaric acid, described in the second of the two papers, as an infringement of the principles set out in the first, because, as he says, it has an "unobtrusive dyad axis," which does not coincide with the axis of the crystal. The only answer is that it has not, as may be seen from Figs. 8, 12, 14, 15 of the paper, or more easily from the model itself. There is no such axis, and, therefore, no infringement.

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The Mechanism of the Cochlea.

In Mr. Wilkinson's letter in Nature of May 12, p. 636, three points are raised upon which I wish to comment.

For the sake of simplicity I described the mechanical conditions occurring when sound waves reach the cochlea in the normal manner by the chain of ossicles. In the case of bone conduction the mechanism of analysis ought to be the same as under other conditions. Bone conduction is the response to a continuous series of uniform waves from a tuning-fork which would produce a corresponding series of vibrations in a resonating system. I cannot agree that the movement "originates *at the basilar membrane*," because the movement depends on the whole resonating mechanism, including the inertia and friction of the fluids.

Damping is the decrease in amplitude due to resistance, and I believe that by using that term Mr. Wilkinson intends to deny any influence due to liquid friction in affecting the note to which the system resonates. In White's "Handbook of Physics" (Methuen and Co., first edition, p. 305), I find "partly closing the mouth [of a resonator] lowers the note." This is an example of friction in a gas affecting the frequency of resonance, which is also seen in the well-known method of tuning organ-pipes. If such an effect is shown with a gas, surely it must be much greater with a liquid in such narrow tubes as those of the cochlea.

With reference to the spiral ligament, I think that the point is unimportant. I merely pointed out the danger of deducing from the size of the ligament the tension on the membrane at rest. To make the point clearer I would suggest the analogy of the size of a pair of hooks supporting a cable. The size of the hooks may not be designed with reference to the tautness of the cable. The cable may be slack, so that the only pull may be that due to its weight; but large hooks may be used, because the cable may have to sustain heavy weights from time to time. I am quite willing to believe that the fibres of the basilar membrane near the *fenestra ovalis* may be more tightly stretched than those near the apex of the cochlea, but that does not necessarily follow from the dimensions of the spiral ligament.

Finally, I wish to emphasise that this correspondence arose in relation to the dimensions of the cochlea and the possibility of such a small structure acting as a resonating mechanism. The point that I wished to bring out was that, on account of its small size, liquid friction will be very great and that this friction may be *one* of the factors in the analysis.

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