

LETTERS TO THE EDITOR

Prompt publication of brief reports of important discoveries in physics may be secured by addressing them to this department. Closing dates for this department are, for the first issue of the month, the

twentieth of the preceding month; for the second issue, the fifth of the month. The Board of Editors does not hold itself responsible for the opinions expressed by the correspondents.

On the Theory of Real Crystals

In a letter with the title *The Plasticity of Rocksalt and Its Dependence upon Water*¹ in connection with a communication of Barnes,² the experimental possibilities for further examining this effect in rocksalt crystals were discussed. In addition I mentioned that, for explanation, the secondary structure of crystals, postulated by Zwicky, need not be considered, because the existence of a secondary structure in rocksalt at present has no theoretical or experimental support. Zwicky³ has attempted to defend his theory, although in connection with the effect of water on the plasticity of rocksalt—the actual subject of my letter—he gives no positive relevant arguments.

Zwicky has not denied the absence of *experimental* proofs for the reality of a secondary structure of rocksalt; on this fact agreement seems to be established. On the other hand he rejects the theoretical paper of Orowan⁴ cited by me, referring to his German publication which has now appeared.⁵ It seems not necessary to discuss this point further, *since Zwicky in his own paper states explicitly, that a proof of a secondary structure of crystals in the mathematical sense does not exist and is hardly possible.*⁶ This frank statement in his German publication (March 16, 1933) is very satisfactory for elucidation of things, as in the letter to *The Physical Review* (April 10, 1933),⁷ Zwicky does not say anything about it, rather denoting it as a particular superiority, that his theory gives a number of quantitative predictions of the structure-sensitive properties of crystals. Nevertheless according to Zwicky's own judgment above mentioned, these predictions are *theoretically wholly unfounded*—as also previously indicated in my letter.

As there is no possibility for a *theoretical* proof of the existence of a secondary structure, the question arises whether the *experimental* facts, mentioned by Zwicky in favor of this conception, are suitable to offer a conclusive proof. Unfortunately macroscopic crystals are systems including always a considerable number of foreign atoms, by which a reliable extrapolation to the pure crystal is prevented. The significance of this lies in the fact, that secondary planes of pure crystals would never be visible. Thus microscopic facts of various metallic crystals till now explained as secondary planes must be realized by assistance of effects of contamination or plastification. For instance Straumanis has admitted expressly this possibility for his results on zinc crystals, consulted by Zwicky in

favor of his theory. Very questionable are also the etching and evaporation observations mentioned by Zwicky; his explanation is by no means the only one.

If it should be experimentally demonstrated that, in the crystals referred to, real regularities exist, their existence *in all other crystals* could never be proved. It is quite possible that here only properties of *certain groups of substances* can be in question, as the structure-sensitive properties examined for these substances (e.g., magnetism) does *not* exist for other crystals at all.

Therefore our own experiments have been undertaken principally on structure-sensitive properties *common to all crystallized matter* as cohesion, plasticity or self-diffusion of crystals. Zwicky has criticized some statements on imperfections of crystals, cohesion and plasticity, which he ascribes to me, but which in that form are not due to me and for which no citations are given. It is a confusion of cohesion and plasticity, when Zwicky asserts that contrary to the "imperfection" theory on increasing the number of imperfection an augmentation of cohesion has been established by experiment. By elimination of the plasticity by help of low temperature the cohesion is in fact *reduced* by chemical or mechanical imperfections, as we have recently demonstrated.⁸ As erroneous is the statement of an independency of temperature of the gliding strength in the neighborhood of the melting point. There is no general law of this kind. As far as I see, here can be considered only experiments by Georgieff and Schmid on bismuth crystals.⁹ But these measurements were not performed at the elastic limit, but on crystals already stretched plastically to some

¹ A. Smekal, Phys. Rev. **43**, 366 (1933).

² R. B. Barnes, Phys. Rev. **43**, 82 (1933).

³ F. Zwicky, Phys. Rev. **43**, 765 (1933).

⁴ E. Orowan, Zeits. f. Physik **79**, 573 (1932).

⁵ F. Zwicky, Helv. Phys. Acta **6**, 210 (1933).

⁶ F. Zwicky, reference 5, p. 213.

⁷ F. Zwicky, Phys. Rev. **43**, 765 (1933).

⁸ W. Burgsmüller, Zeits. f. Physik **80**, 199 (1933); **83**, 317 (1933); K. Steiner and W. Burgsmüller, Zeits. f. Physik **83**, 321 (1933); A. Smekal, Zeits. f. Physik **83**, 313 (1933); Phys. Zeits. **34** (1933).

⁹ M. Georgieff and E. Schmid, Zeits. f. Physik **36**, 759 (1926).

0.3 percent, which in the neighborhood of the melting point even during the drawing experiment could have intensive recrystallization.

According to Zwicky's theory of secondary structure the low macroscopic gliding strength of the crystals would correspond to the real molecular stresses along that pair of secondary planes operating as gliding plane. In his German publication Zwicky gives a calculation of these stresses for the rocksalt lattice.¹⁰ According to the "imperfection" theory these molecular stresses should be at the elastic limit about 1000 times higher, corresponding to the stresses calculated by the theory of the ideal crystal lattice. Thus, here is given the possibility of an experimental decision

between both theories. As we have shown by experiments, the actual molecular stresses are in fact already at the elastic limit much higher than the macroscopic shearing strength and of the order of those following the ideal lattice theory.¹¹

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¹⁰ F. Zwicky, reference 5, p. 212.

¹¹ A. Smekal, Phys. Zeits. **34** (1933); report at the Gauvereins-meeting of the Deutsche Physikalische Gesellschaft in Freiberg, June 10, 1933.

Band Spectra Measurement of Mass

The masses of the elements may be measured by three general methods: (1) the mass-spectrograph gives a method in which the motion of an ion is calculated in the presence of an electric or magnetic field or both, and from this the mass deduced; (2) a value for the mass of an element may be derived from artificial disintegration experiments provided the masses and energies of the other constituents of the reaction are known and (3) band spectra furnish a method from which the ratio of the masses of an isotope may be determined.

For the case of Li⁷ the values of the masses given by methods (1) and (2) are in excellent agreement when we use the reaction $\text{Li}^7 + p \rightarrow 2\alpha$. The agreement is not quite as satisfactory for $\text{Li}^6 + \text{H}^2 \rightarrow 2\alpha$.¹ The ratio of the masses of the lithium isotopes from band spectra is in disagreement with the ratio from Costa's and Bainbridge's mass-spectrograph data and that from method (2).²

In the case of Be the first two methods do not agree when we use the reaction $\text{Be}^9 + \alpha \rightarrow \text{C}^{12} + n$ to calculate the mass of Be as neutrons of too high an energy are occasionally observed.³

For the case of boron, methods (1) and (3) agree very closely, whereas the mass obtained by the second method from the reaction $\text{B}^{11} + p \rightarrow 3\alpha$ are lower.⁴

The discrepancies between the masses determined by the mass-spectrograph and the masses computed from artificial disintegration experiments may arise from the production of γ -rays or from one of the nuclei entering into the reaction being in an excited state with long mean life. The excitation might arise from a previous collision.

With these data in mind we have examined the band spectrum theory on which these mass ratios are determined to see whether these discrepancies are of a fundamental nature or not.

Kronig⁵ has shown that the effective mass M that appears in the relationship MR^2 should be replaced by

$$MR^2 + (m/2)[\xi_1^2 + 2\zeta_1^2 + \Sigma_r(\xi_r^2 + \eta_r^2 + 2\zeta_r^2)]$$

$$M = M_1 M_2 / M_1 + M_2$$

m = electronic mass and ξ_1 , etc., the electronic coordinates. This substitution will change the observed moment of

inertia I by as much as 1 part in 10^{-4} . For the molecule H^2Cl^{35} where 4/5 of an electron has been taken to be associated with the H^2 atom, this will reduce the ratio factor by 8 parts in 10^{-5} .

There is one other effect which may play a rôle and that is the change in the effective radius of the nucleus as the mass of the isotope is changed. The electronic isotope effect in band spectra has been observed by Jenkins and McKellar⁴ for BO and they find the surprisingly large value of 0.33 cm^{-1} . For the H_2^2 molecule, the discussion may be conveniently divided into two different calculations. If we suppose that each electron is associated with a definite nucleus, as in the method of Heitler and London, then we have the perturbation arising from the electrons being at the position of its own nucleus—the probability of this event is to a first approximation measured by $\psi^2(0)$ —and the perturbation arising from the electrons being at the position of the other nucleus—the probability of this latter event is to a first approximation given by $\psi^2(R)$. For the H_2^2 molecule this change is negligible. For the H^2Cl^{35} molecule, preliminary calculations indicate that ρ may be changed by as much as 1 part in 10^{-5} by this perturbation. If we use these values for the contribution of these two perturbations we find a value of the mass of H^2 of 2.01360 with the same limits of error as those reported by Hardy, Barker and Dennison.⁵ This is in good agreement with

¹ Cockroft and Walton, Proc. Roy. Soc. **A137**, 229 (1932); Lewis, Livingston and Lawrence, Phys. Rev. **44**, 56 (1933).

² Bainbridge, Phys. Rev. **44**, 56 (1933); Jenkins and McKellar, Phys. Rev. **44**, 325A (1933).

³ Bainbridge, Phys. Rev. **43**, 367 (1933); Kurie, Phys. Rev. **43**, 771 (1933).

⁴ Cockroft, Phys. Rev. **44**, 314A (1933); Kirchner, Naturwiss., June 23, 1933; Aston, Proc. Roy. Soc. **A115**, 487 (1927); Jenkins and McKellar, Phys. Rev. **42**, 464 (1932); Oliphant and Lord Rutherford, Proc. Roy. Soc. **A141**, 259 (1933).

⁵ Kronig, Zeits. f. Physik **46**, 814 (1928); **50**, 347 (1928).

⁶ Hardy, Barker and Dennison, Phys. Rev. **42**, 279 (1932).