atomic scattering power with temperature, an important effect which would reduce his deduced 'X-ray active' frequency from 160 cm.$^{-1}$ (his figure) to 100 cm.$^{-1}$. The latter does not correspond to a Raman frequency, and is in fact altogether below the range of optical frequencies for NaCl. It corresponds to an angular frequency $1 \cdot 89 \times 10^{13}$, but no reliance can be placed even upon this figure (although it would agree well with the thermal theory), since the intensity measurements are obviously extremely rough and limited in extent.

(2) He assumes that Laue spot and thermal spot intensities should vary differently with crystal volume. This is not the case; both are proportional to the crystal volume, proper experimental precautions being taken and corrections made[7]. Venkateswaran has compared diffuse spots given by $\lambda = 0 \cdot 710$ with Laue spots due to $\lambda = 0 \cdot 417$, $0 \cdot 592$ and $0 \cdot 401$ A., using a thick crystal for which no differential absorption corrections are made. He states that there is no variation of the Laue/diffuse spot intensities, as indeed the thermal theory predicts, but his photographs cannot be accepted as serious evidence either way.

(3) He assumes that the velocities of elastic vibrations are identical in all directions and irrespective of polarization (Raman also states that such a simplified treatment "should be a fair approximation to the truth"; *ibid.*, p. 344). A recognition that this is far from being true is vital to a proper comparison of theory and experiment. Venkateswaran's measurements of sharpness, etc., are in reasonable agreement with the correct version of the thermal theory, though not with the approximate theory that he assumes.

(4) His assumption that the thermal theory predicts the same law of intensity decay with angle of diffraction for diffuse as for Bragg reflexions is incorrect. Temporary displacements of atoms naturally affect the small-spacing and high-order reflexions much more than those of large spacing or low order. This was well confirmed by Laval's careful observations[8]. He found for potassium chloride at 290° C., using molybdenum $K\alpha$ monochromatized radiation and measuring the diffuse spot intensities (by the ionization spectrometer) at a constant angular displacement from the Bragg reflecting positions:

	002	004	006	008	0010
Bragg intensity	100	20	7	3	1
Diffuse intensity	100	50	33	21	12

For planes of similar structure factor but different force constants the diffuse spot intensities may bear little relationship to the Bragg intensities. This was also confirmed by Laval.

(5) It follows from (4) that Venkateswaran was wrong in expecting that, according to the thermal theory, the 111 : 222 diffuse spot intensity ratio would be equal to that of the Bragg spots. Moreover, the method he used of obtaining the diffuse reflexions by transmission through a thick crystal of an "effective X-ray wave-length maximum" of 0·46 A. was quite unsuitable for comparison of intensities. In fact, Fig. 3 (plate 29) of his paper shows that some molybdenum characteristic radiation was also being transmitted, and this would certainly invalidate his measurements. Accurate intensity comparisons are only possible using monochromatic radiation, proper allowance being made for 'unwanted' components.

(6) In order to explain the drift of the diffuse maxima towards the Laue spots, Venkateswaran postulates 'phase-waves' parallel to certain of the {311} planes. This drift is a natural consequence of the thermal theory, which predicts its magnitude and direction correctly[9] and without any extraneous assumptions.

I do not suggest that the thermal theory has reached a state of perfection and is now beyond criticism. That is not the case. It may, however, be claimed that the experiments described by Venkateswaran, in so far as the results are of value, confirm and do not contradict the thermal theory. From this point of view, therefore, there is no ground for criticism of the Born lattice theory.

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Regulation of Experiments on Living Animals

AT the end of June a somewhat rare event will take place. The Home Office will make an appointment under the Cruelty to Animals Act, 1876, which regulates the practice of experiments on living animals.

An inspector under the Act exercises an important function, not, indeed, as a policeman, but in advising research workers as to the interpretation of their obligations in particular cases. In view of the number of experiments licensed under the Act, direct supervision is not practicable on any material scale, but I assume that the majority of research workers do not desire to contravene the regulations, and that the inspector's guidance will in general be effective.

The Act lays down a compromise between the claims of science, on one hand, and the rights of animals, on the other; the line drawn in the practical application of the compromise must inevitably be an arbitrary one. Until somebody can lay down clear-cut rules which will command general assent, the best that can be done is to make as fair a compromise as possible. This calls for exceptional impartiality, and since a man with the indispensable scientific training will necessarily have a predisposition in favour of science, it is important that he shall also have an offsetting predisposition in favour of the other party in the compromise. This could be ensured by requiring candidates for the post to prove that they have in the past been effectively associated with some effort to better man's treatment of animals, wild or domestic. Preferably they should also have veterinary knowledge.

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