I wish to thank Prof. T. Futagami for valuable discussions.

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The quantity 2φ defined by Yoneda for his own calculations differs from my quantity 2φ, the definition of which he quotes earlier, by 180°. Consequently his result 132° ± 3° becomes, in my notation, 48° ± 3°, which is not very different from my 52°. Whether it is superior is very debatable. Yoneda's value increases the discrepancy from the observed spike intensities (relative to that at first order) in the second, third, and fourth orders, without gaining any satisfactory agreement at fifth order, for which he still obtains a stronger spike than at third order, instead of an absent one. I supposed another factor to be present, giving a general falling off of intensity at higher order. Yoneda's calculation does not dispose of the evidence for such a factor, but if such a factor were present, his least squares analysis is less appropriate than my method of calculation, the essential point of which is to give great weight to the location of a minimum (a zero, according to the theory) near the third order. The most remarkable result is the smallness of the change which his method of calculation brings about.

The bond-lengths 1·45 Å. or 1·64 Å. which Yoneda calculates are members of a series of bond-lengths (between carbon atoms and an assumed monolayer of impurity atoms) which would all give the same diffraction spikes except at zero order, namely :

1·399, 1·453, 1·640, 1·719, 1·955, 2·046 ... ...

for Yoneda's value ($2\varphi = 48°$) or

1·395, 1·457, 1·633, 1·723, 1·950, 2·053 ... ...

for my value ($2\varphi = 52°$)

while my less preferred value ($2\varphi = 67°$) would give bond-lengths about another 0·012 Å. alternately smaller and greater than the latter list. The associated numbers for the impurity to give minimal spike intensity at zero order are in all three cases:

2, 4, 8, 10, 14, 16 ...

It is clear on this basis that if the impurity were to be identified as nitrogen ($Z = 7$), the third member of this series should be selected, and, as Yoneda remarks, this gives a credible bond-length, namely, 1·640 Å. according to Yoneda, or 1·633 Å. or 1·620 Å. (with preference for the former) according to me.

The basis for Yoneda's revision of Caticha-Ellis and Cochran's estimate of atomic concentration of impurity from 8×10^{-4} to 8×10^{-3} is not apparent: no acceptable change in δ should alter the result by so much. On the contrary, Caticha-Ellis and Cochran's estimate must be essentially independent of the nature of the defect, within its acceptable range, so that Kaiser and Bond's nitrogen contents are adequate to produce the diffraction spikes.

While this gives a credible interpretation of the 'spike-producing' defects, it must be borne in mind that the multiplicity of alternative magnitudes of displacement giving similar diffraction results makes the diffraction evidence alone incapable of discriminating against other possible models (such as one with nitrogen atoms in two layers). The matter can scarcely be settled before we have X-ray diffraction, infra-red absorption, and spin-resonance measurements all on the same diamond.

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BIOPHYSICS

Proposed Use of Magnetic Fields in Electron Microscopic Radioautography

The use of strong magnetic fields has been proposed as a possible way of increasing resolution in radioautography[1]. It was reasoned that a sufficiently strong magnetic field would bend the path of emitted β-particles into a circle of arbitrarily small radius, thereby restricting the resulting radioautographic grains to a small zone around the source. In recent reports[2,3], Harford and Hamlin, using the same argument, attempted to improve the resolution of electron microscopic radioautographs by placing the specimens in a 10-kilogauss magnetic field during exposure. They could not demonstrate such an improvement; but they attributed a considerable increase in the number of exposed grains to the effect of the field on the β-particles from tritium.

A simple calculation shows, however, that magnetic fields at present available can have little influence on the path in emulsion of low-energy β-particles. Tritium emits electrons of approximately 18-keV. maximum energy and 5·5-keV. average energy. Let us take the case of a 10-keV. electron (in the high-energy side of the spectrum, but with still a fairly high probability of occurrence). For such an electron moving perpendicularly to the lines of force of a uniform magnetic field the product $HR = 3{\cdot}386 \times 10^2$ gauss cm. (ref. 4) (where H = magnetic field intensity and R = radius of curvature). For a commonly available field of 10^4 gauss $R \simeq 3{\cdot}4 \times 10^{-2}$ cm. = 340μ. The maximum range of a 10-keV. electron in material of density 1 is calculated to be of the order of 2·5μ (ref. 5). In emulsion it is probably about one half this value. On such a short path a curvature of radius 340μ would have no detectable effect. As the electron slows down, the radius of curvature will decrease, but, even for a 1-keV. electron with a calculated range in water of 0·053μ, the radius of curvature would still be about 100μ.

We may ask what fields would be needed to have a useful influence. A resolution of the order of 0·2–0·3μ can be theoretically expected (unpublished results), and experimentally demonstrated[6], when a monolayer of a fine-grained emulsion is used in electron microscopic radioautography[7]. We can ask, therefore, that the field be of sufficient strength to give a 10-keV. electron a curvature of 0·1μ radius. Taking $HR \simeq 3{\cdot}4 \times 10^2$ gauss. cm., we get, under these conditions, $H = 3{\cdot}4 \times 10^7$ gauss. This is approximately one hundred times stronger than any permanent field yet achieved. Since the change in electron