Impurities in diamonds

from John Walker

RESULTS obtained by Melton and Giardini (page 309 of this issue of *Nature*) suggesting that oxygen not nitrogen might be the dominant impurity in diamond pose a problem in the light of previous work. In 1936, Robertson *et al.* (*Proc. R. Soc. Lond.*, **A157**, 579–593) classified diamonds into two types by their optical absorption spectra. Kaiser and Bond (*Phys. Rev.*, **115**, 857–863; 1959) using mass spectroscopy, showed that nitrogen impurity was the microscopic basis of this classification; type I diamonds contained nitrogen, type II did not. The nitrogen concentration correlated with the optical absorption lines noted by Robertson *et. al.* Lightowlers and Dean (*Diamond Res.*, 21–25; 1964), using activation analysis, confirmed Kaiser and Bond's results. In some diamonds the nitrogen is paramagnetic, and can be detected unambiguously by spin resonance. Thus there is no doubt that nitrogen is a major impurity in diamond—up to 0.3%—a massive concentration for an impurity in a crystal.

Kaiser and Bond also found small amounts of hydrogen and oxygen. Oxygen has been confirmed by Sellschop (*Diamond Res.*, 35–41; 1975) using activation analysis.

One of the problems in determining the impurity content of diamond is that there are often trapped inclusions of other minerals. These are important, particularly to the geologist, because they indicate the growth environment of the diamond; but they may give a misleading picture of which elements can genuinely enter the diamond lattice as substitutional or interstitial impurities. Kaiser and Bond, and Lightowlers and Dean used stones without visible inclusions. Even so, Sellschop has shown that all diamonds contain submicroscopic inclusions, and these account for most of the oxygen impurity.

Using mass spectrometry, Melton and Giardini (*Amer. Mineral.*, **59**, 775–782; 1974 and **60**, 413–417; 1975) found that some macroscopic inclusions contain gases—hydrogen, methane, nitrogen, carbon monoxide and water.

The most abundant element was hydrogen, followed by oxygen and nitrogen. The results are not in conflict with previous studies, because hydrogen cannot be detected by activation analysis, and nitrogen and oxygen need special techniques. The most comparable work is that of Kaiser and Bond, but even here there is no conflict. The latter selected "inclusion-free" diamonds, whereas Melton and Giardini chose crystals containing inclusions, and studied the inclusions.

Thus the picture of impurities in diamond was complicated but consistent. The geologist studies the inclusions; the physicist, the genuine diamond lattice. In particular the physicist, whether he is concerned with optical, electrical, mechanical or thermal properties, knows that type I and type II diamonds have quite different properties, because of their differing nitrogen content.

This consistent picture has been upset by Melton and Giardini's latest paper. They used "inclusion-free" diamonds, and instead of crushing them as previously, they graphitised them at 2,000 °C like Kaiser and Bond. The results are surprising. "The gases released from the diamond, in decreasing order of abundance, were CO, H_2, H_2O, CO_2, N_2, CH_4, and Ar. The atomic weight percent is O=59.2%, C=28.1%, H=10%, N=2.5% and Ar=0.2%. These data are consistent with the gases released by the crushing of numerous other diamonds *in vacuo*."

Since nitrogen is currently thought to be the dominant impurity in diamond, how can we explain this discrepancy? Melton and Giardini suggest nitrogen contamination from the carbon crucible used by Kaiser and Bond. But this is unlikely because Kaiser and Bond's results are internally consistent—the nitrogen concentration correlated with optical absorption in their crystals. And Lightowlers and Dean, using a different technique, got the same result.

A possible explanation is that the diamonds came from different sources. This is known to affect impurity content. In any case, the fact that Melton and Giardini's diamonds are oxygen-rich, surprising though it may be, does not contradict Kaiser and Bond's results on nitrogen. Unfortunately, Melton and Giardini do not give the concentrations of oxygen and nitrogen they found. My estimates based on their data suggest that both lie within the range of accepted values; but it would be better to have the authors' own figures.

One hopes that Melton and Giardini will extend their graphitisation work to diamonds from other sources, and measure the optical spectra before graphitisation, to check Kaiser and Bond's correlations. If the same diamonds could also be studied beforehand by the non-destructive technique of activation analysis, to compare results, the puzzle would probably be resolved. Diamond researchers will await further results with interest.

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conferring differential stability. T. Humphreys (University of Hawaii) reported that on fertilisation of sea urchin eggs messenger translation is increased—the messengers are stable but their poly(A) turns over rapidly. Using the same system, G. Giudice (Institute of Comparative Anatomy, Palermo) has shown that although capping does not cause the increase in translational efficiency it may be involved in mRNA selection.

R. A. Laskey (MRC Laboratory of Molecular Biology, Cambridge) has examined the effect on endogenous protein synthesis of injecting polysomes and mRNA into *X. laevis* oocytes. He maintains that oocytes do not have 'extra translational capacity' and that by choosing conditions that accurately reflect the amount of protein synthesised the endogenous protein synthesis decreases competitively with added exogenous mRNA. There is no competition when the messenger is injected with its own ribosomes. Laskey concludes that the amount of protein synthesised is regulated by a component of normal polysomes and not by messenger availability.

Virus infection

The series of lectures on the control of translation during viral infection was similar to those on masked messengers in development in that many interesting systems were described without providing much insight into the mechanism of control. In general, host mRNA translation is suppressed at the expense of viral protein production. The herpes simplex system was described by C. M. Preston (MRC Virology Unit, Glasgow). He has shown that the effect of this virus on tissue culture cells can be reproduced in a lysate of the cells. The lysate can also respond to exogenous mRNA, but its response is decreased if the lysate is prepared from infected cells. The lesion can be traced to the ribosomes of the infected cells, since proteins washed off reticulocyte ribosomes by salt treatment restore the translational capacity. R. E. Thach (Washington University) has shown that EMC RNA can 'out-compete' host mRNAs but that the competition is relieved by an excess of the initiation factor corresponding to rabbit IF-E6 (IF-M3). L. Carrasco (ICRF, London) has found that after infection with picornavirus, changes take place in the cell membrane which impair the sodium transport system, leading to an increase in the cell concentration of sodium ions and a decrease in potassium. Experimental evidence has confirmed his suggestion that increased sodium concentration favours the translation of viral, rather than host, mRNAs. It may be that at these altered salt conditions binding of initia-

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