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AUTHOR'S REPLY: EXTINCT RADIOACTIVITIES AS TRAPPED RESIDUALS OF PRESOLAR GRAINS

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ABSTRACT

Trivedi's attempted disproof of presolar grains as carriers of special ^{129}Xe* in meteorites is rejected on three grounds: (1) the high-T losses of ^{129}Xe* and ^{127}I are tightly correlated, (2) the sharply isochronous meteorites are partially differentiated but not truly metamorphosed, and (3) evidence that much of solar ^{129}Xe was trapped in presolar grains has strengthened the plausibility of the presolar-grain picture.

Subject headings: interplanetary medium — meteors and meteorites — nucleosynthesis

In 1974 I decided to develop my picture of the precipitation of anomalous grains in expanding supernova interiors and their survival in solar-system objects because I suspected that astrophysics was about to experience a revolution in ideas related to the formation of the solar system. The discovery of the $+\delta^{16}$O anomaly (Clayton, Grossman, and Mayeda 1973) still seems to me to require that several percent of the oxygen-bearing grains in the solar nebula had in fact formed long before in the ejecta of stellar He or C burning before such ejecta could mix with interstellar gas. It therefore seemed natural to examine other consequences of that clue. Clayton (1975*a*) specifically described the most exciting but controversial of these implications—a wholly new interpretation of xenon isotopic anomalies and of the chronology of the formation of the solar system. Clayton (1975*b*) showed how the concept of extinct radioactivity should then be enlarged to include even species with half-lives measured in years, such as ^{22}Na, and went on to predict several leading possibilities of that picture. Clayton and Hoyle (1976) carried the argument to nova dust and its accretion by the solar system.

Trivedi (1977) has questioned this in the preceding paper, concentrating on ^{129}Xe* and alleging that its correlation with ^{127}I could not survive thermal metamorphosis. That argument seems to me to be generally appreciated already, and the puzzle of the correlation's survival of metamorphosis can be found in old papers.

One major point of uncertainty concerns the circumstances of the physical release of the iodine and the ^{129}Xe*. The latter is certainly *not* released by ordinary diffusion, because Xe and I will not diffuse at the same rate. An initially high loss rate at a given temperature, say 1000° C, may instead cease abruptly and remain zero until the temperature is raised. In that case one cannot conclude that a mineral collection that loses 50%, say, of its ^{129}Xe* in 1 hr at temperature T will lose 75% in 2 hr, or even that it will lose 75% in 10^6 hr. The relevant experiments have not yet been performed. Both I and ^{129}Xe* seem to be trapped in cohost cages that do not release until a threshold temperature has been reached (Hohenberg and Reynolds 1969). It is just this property that makes the correlation so persistent. It is incorrect to allege that the experiment "indicates only the volatility of both I and Xe at high temperature." Wetherill (1975), whom Trivedi refers to on other matters, expresses it thus: "... metamorphosis and other chemical differentiation occurred at a later time. Under this interpretation the record of ^{129}I–^{129}Xe formation intervals must be very resistant to these later processes and is able to 'see through' these events. At first this may seem surprising due to the volatile nature of both iodine and xenon. Since the age is based on the correlated high temperature ($>1000°$ C) release of xenon, however, it could be that the sites in which this iodine and xenon are found are very retentive. These sites have not yet been identified in a mineralogical sense." In support of this conclusion, one recalls that Hohenberg and Reynolds (1969) showed that preheating of Abee to 1200° C did not destroy the ^{127}I–^{129}Xe* correlation above 1150° C, despite driving out 90% of the xenon. Even higher $T(>1300°$ C) fractions were depleted without destroying the correlation. They concluded that *quantitative* retention is not necessary. Indeed, the absolute amount of special anomalous ^{129}Xe* is quite variable from meteorite to meteorite, and from matrix to chondrule. It is only the ratio of high-T ^{129}Xe* to high-T ^{128}Xe* generated from ^{127}I$(n, \gamma)^{128}$I that is relatively constant through the classes of chondrites. It is the cohost cages which enable some of these correlated nuclides to survive high temperatures. There simply is not enough experimental information to allege that the high-T correlation cannot survive metamorphism, nor is there enough information to be sure that estimates of metamorphic temperatures are accurate for millions of years in a warm planetesimal.

A second major point of uncertainty is whether metamorphism in the classic sense has actually occurred. Many stony meteorites show features

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resembling breccias composed of components having differing degrees of differentiation. Fredriksson, de Gasparis, and Rambaldi (1975) report that the class of ordinary chondrites, including a typical H5, contains an ultrafine matrix component rich in volatiles and primitive in character. They claim that their "results clearly demonstrate that the chondrites were not equilibrated during a late metamorphic episode." Wilkening (1976) reports carbonaceous chondritic fragments in 16 meteorites, including two howardites, Jodzie and Kapoeta. She suspects these as carriers of the rich xenon gas. If Jodzie can have fine C1 inclusions scattered throughout its presumably metamorphosed interior, who can say that the stony meteorites do not also contain finely divided cohost cages for this special ^{129}Xe*–^{127}I correlation? The two achondrites bearing ^{129}Xe* in Trivedi's Table 1 are both aubrites, which, "while highly differentiated, display some properties which indicate that they were not formed by igneous processes" (Wasson 1974, p. 24). And of the occurrence of ^{129}Xe* within iron meteorites I would again quote Wasson (1974, p. 71): "The silicate portions of group I AB irons are chondritic and appear to have escaped any appreciable melting." On the other hand, Pasamonte and Petersburg, which contain no ^{129}Xe*, are eucrites, which "appear to have resulted from igneous differentiation" (Wasson 1974, p. 24), and are therefore consistent with the expected fate of presolar grains under true igneous metamorphism. Perhaps the question should be put the other way: where would the eucritic ^{129}Xe* be if ^{129}I, which is not affected by metamorphism, were actually extant? The presolar-grain picture may even be superior!

Perhaps the specific issue cannot be settled with ^{127}I–^{129}Xe* data alone. The two years since Clayton's (1975*a*) paper have witnessed many new discoveries of anomalies, and the total picture is complex and full of poorly understood but exciting information. Information on mineralogical trapping sites is only now coming into existence. Presolar grains seem to me to certainly play a role in some, but perhaps not all, anomalies. It is to this total picture that we must look for guidance.

Drozd and Podosek (1976) have made the remarkable discovery that the trapped-xenon component of the Arapahoe chondrite is deficient by almost a factor 2 in ^{129}Xe, although the special ^{129}Xe*/^{127}I correlation has the largest ratio ever discovered. The large ^{129}Xe*/^{127}I ratio requires on my picture (Clayton 1975*a*) that a higher-than-average fraction of the iodine-bearing grains assembling into Arapahoe were presolar. That will come about naturally if most of the presolar grains are still unvaporized but nonetheless largely removed from the region of Arapahoe's assembly. Drozd and Podosek (1976) adapt my model by postulating that the presolar grains had not yet vaporized and therefore had not yet increased ambient ^{129}Xe at the time Arapahoe assembled. They conclude that gas-dust segregations must have been common in the early solar system and appeal, as I suggested, to less common times and places from which presolar grains had been largely removed before vaporization. The remaining unvaporized grains could still leave a large ^{129}Xe*/^{127}I. It is noteworthy that the picture I advanced has already proved capable of explaining this major new discovery. Their argument would quantify my own by showing that much (>40%) of the ^{129}Xe inventory of the solar system was trapped in dust, which in turn requires that something like half of the iodine is also trapped in presolar dust that was originally condensed in supernova ejecta. In my notation (Clayton 1975*a*),

$$\langle {}^{129}\mathrm{Xe}^*/{}^{127}\mathrm{I} \rangle_G r_{\mathrm{I}} \geq 0.5 \,. \tag{1}$$

If so, Arapahoe should be deficient by a factor of about 2 in its iodine concentration relative to similar chondrites, even though marginally richer in the presolar component. This is borne out by observation, as Drozd and Podosek (1976) themselves find that [I] = 13 parts per billion (ppb) in Arapahoe, in contrast to 32 ppb in Bjurbole, which is more representative of L-type chondrites (Mason 1971).

I cheerfully acknowledge that this controversial application of presolar grains may be incorrect. It could turn out that the ^{129}I actually did exist in the solar system, as Trivedi reemphasizes. If so, it may even be that this ^{129}I is also carried primarily by the presolar grains, just as the ^{129}Xe appears to be. I think it is still too early to decide these questions, and I resist premature attempts to regard them as settled. I am content to repeat my original purpose. I have presented an original interpretation of extinct radioactivities and their applications. Like any theoretical picture, its merit comes from its extension of the framework of our interpretations.

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